The Non-Monotonic Political Effects of Resource Booms*

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Abstract

This study uses variation in mining transfers and mineral production among Peruvian municipalities to analyze how resource booms affect mayoral reelection, political competition, public-good provision, municipal employment, and citizens' well-being. Although this topic has attracted increasing scholarly interest, existing studies document heterogeneous effects of resource booms on public-good provision and living standards. Several theoretical models allow these responses to vary with the size of resource rents, whereas much of the empirical literature has relied on linear specifications. By examining Peru's mineral-price boom and the statutory rules governing the distribution of mining transfers, I estimate district and year fixed-effects models using a municipal panel for 1996–2010. I find convex relationships between mining transfers, mayoral reelection, and the dispersion of candidate vote shares. I obtain similar patterns when using alternative production controls and restricting the analysis to non-producing recipient districts. The results for public-good provision, municipal employment, and household income are also consistent with responses that vary with the size of the transfer windfall, while the corresponding evidence for household consumption is weaker. Taken together, the findings are consistent with transfers expanding incumbents' fiscal resources while also raising the value of holding office and the competition for it.

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1 Introduction

The abundance of natural resources has long been associated with poor economic and political performance. Anecdotal evidence and cross-national studies suggest that many resource-rich countries fail to transform their natural wealth into higher levels of well-being. These countries also appear more vulnerable to economic and political pathologies that can undermine growth and development.[1] These patterns have led scholars to describe the phenomenon as a "resource curse" (Ross, 1999). Theoretical work has identified several channels through which resource wealth may shape economic and political outcomes (Mehlum et al., 2006; Robinson et al., 2006; Torvik, 2002). A growing empirical literature has examined the consequences of resource wealth, but the relative importance of these channels remains an open question.

Political outcomes provide a particularly useful setting in which to examine these mechanisms.[2] Existing studies have provided important evidence on the links between resource abundance, governance, and democracy (Jensen and Wantchekon, 2004; Morrison, 2009; Robinson et al., 2006). The direction and magnitude of these relationships nevertheless vary across settings and measures.[3] Despite their differences, most explanations assign a central role to the behavior of politicians and political elites. For instance, Jensen and Wantchekon (2004) emphasize the incumbent's discretion over the distribution of natural-resource rents as a key mechanism linking resource abundance to institutional and democratic performance.

In this paper, I shed light on these issues by studying how a mineral-resource boom affects the electoral behavior of local politicians and what these responses imply for citizens' well-being. More specifically, I examine how mining transfers affect mayoral reelection and political competition, paying particular attention to two instruments available to politicians: public-good provision and municipal employment. I then analyze how these political and fiscal responses relate to household living standards.

To do so, I exploit variation in mining transfers and mineral production across Peruvian districts and over time. In the empirical analysis, mining transfers include only Mining Canon and mining royalties; Oil Canon and other canon categories are not part of the treatment. Statutory allocation rules distribute these revenues not only to the municipality governing a producing district but also to other municipalities in the same province and region. Combined with the international mineral-price boom, these rules generated large and heterogeneous changes in local fiscal resources. I compare within-district changes in political and economic outcomes across different levels of transfer

[1] Resource abundance has been associated with the "Dutch disease"—an appreciation of the real exchange rate that contracts the tradable sector—as well as slow economic growth, unemployment, low savings, high external debt, unstable export earnings, and limited export diversification. For overviews, see van der Ploeg (2011), van der Ploeg and Poelhekke (2017), Venables (2016), Badeeb et al. (2017), and Cust and Poelhekke (2015).

[2] For an overview, see Deacon (2011).

[3] The disagreement is particularly clear in the literature on democracy. Ross (2001) was the first of several studies to document a negative association between natural resources and democracy. In contrast, Haber and Menaldo (2011) question this relationship and find that, under some conditions, natural resources are associated with better democratic outcomes.

exposure and distinguish districts that host mineral production from non-producing recipients.[4]

Peru offers an especially useful setting in which to examine the political consequences of resource abundance. First, the country is one of the world's largest mineral producers: it ranks second in copper and silver, third in zinc and tin, fourth in lead and molybdenum, and sixth in gold.[5] Second, there is substantial spatial variation in the minerals produced across the country. Third, Peru experienced an extraordinary mineral boom during the period under study. Between 1996 and 2010, the real value of mineral production increased more than fourfold, from US$1.35 billion to US$5.67 billion at 2001 prices. Over the same period, real mining transfers received by district governments rose from 21 million to 2.49 billion PEN at 2001 Lima prices and peaked at 3.95 billion PEN in 2007. This expansion created a small group of municipalities whose budgets increased dramatically. Fourth, the statutory allocation rule distributes part of the taxes and royalties generated by mining to non-producing districts in the same province and region. This feature helps distinguish the fiscal-transfer channel from changes in the local economy associated with mineral production.[6] Finally, Peru's local political arena is highly fragmented and only weakly connected to national political parties. Strategic interactions between local politicians and national party leaders are therefore less likely to determine how mining transfers are distributed.

I construct a dataset of mineral production, intergovernmental transfers, electoral outcomes, public-good provision, and local-government characteristics for 1996–2010. I focus on this period because the prospect of immediate mayoral reelection is central to my conceptual framework, and a subsequent reform debate may have changed that prospect before the prohibition took effect. My main specification is a district and year fixed-effects model in which mining transfers are a continuous treatment. I consider both average transfers over an electoral cycle and transfers received in the election year. Because production may respond to the price boom and affect local outcomes directly, I complement production controls with estimates for recipient districts that never produce minerals during the sample period. These districts receive statutory transfers without hosting extraction and therefore provide the most direct separation of the fiscal-transfer channel from the local effects of mineral production.[7]

The fixed-effects estimates indicate a convex relationship between election-year transfers and mayoral reelection, and between average transfers over the electoral cycle and the dispersion of

[4] Ideally, the entire variation in mining transfers would reflect international prices. In practice, mining companies may have expanded production or opened new operations in response to higher prices, and these changes may have affected local economic and political outcomes directly. I therefore combine production controls with specifications that exclude all producing districts rather than treating realized transfers as mechanically exogenous.

[5] For details, see MINEM (2013).

[6] Conceptually, an exogenous increase in mineral prices can raise mining transfers without a contemporaneous change in mineral production. In that case, politicians are affected through the municipal budget. Higher prices may also induce production changes that increase employment and income or, conversely, generate environmental degradation, crime, and conflict. These channels need not affect political outcomes in the same direction.

[7] An alternative design would predict transfers using pre-boom production and international mineral prices. This approach is incomplete in the Peruvian setting because non-producing municipalities receive a substantial share of mining transfers through province- and region-level allocation rules. It would therefore discard much of the relevant treatment variation. I instead use the observed transfer allocation, control for production, and report separate estimates for non-producing recipients.

candidate vote shares. At low and average transfer levels, higher transfers are associated with lower mayoral reelection and less dispersed vote shares, but the estimated marginal effects become less negative as transfers increase. In both cases, the estimated marginal effects change sign only at transfer levels far above the 99th percentile. The reelection pattern becomes stronger when I restrict the analysis to non-producing recipients; the political-competition coefficients retain the same direction but become less precise.

I also find patterns consistent with diminishing marginal effects for several measures of public-good provision, selected categories of local infrastructure and road investment, and municipal employment. A similar pattern appears for household income, while the evidence for household consumption is weaker. Taken together, these results point to short-run improvements in some dimensions of citizens' living standards rather than a uniform effect across outcomes.

These results can be interpreted through recent theoretical work on the political economy of resource booms (Caselli, 2006; Caselli and Tesei, 2016; Caselli and Cunningham, 2009). This literature emphasizes that competition among political elites over the appropriation of natural-resource rents may generate non-monotonic political responses. On the one hand, additional rents provide incumbents with fiscal resources that can be used to influence electoral outcomes through public-good provision or clientelism. On the other hand, the same rents increase the value of holding office and may attract stronger political challengers. Which force dominates depends on the size of the rents and the incumbent's ability to convert fiscal resources into political support.

Taken together, the results show that a linear approximation can conceal important features of resource booms. Although the theoretical literature has long recognized the possibility of non-monotonic responses, empirical work has largely emphasized constant-slope specifications. To the best of my knowledge, this paper is among the first to examine how the marginal relationship changes with the size of a resource boom using subnational variation.

This paper contributes to the empirical literature on the economic and political effects of natural resources in several ways. First, I treat resource booms as complex phenomena involving changes in both production and natural-resource rents. Whereas much of the recent empirical literature focuses on rents (Brollo et al., 2013; Caselli and Michaels, 2013; Monteiro and Ferraz, 2012), I also examine the role of production and exploit the statutory allocation of transfers to non-producing districts to distinguish the fiscal windfall from local extraction. Second, I complement the primary fixed-effects analysis with a formal sensitivity exercise that relaxes the exclusion restriction for an imperfect instrument. Third, the analysis helps address an important puzzle in the existing literature: large fiscal windfalls often produce few detectable improvements in public services or household well-being (Caselli and Michaels, 2013; Monteiro and Ferraz, 2012). Political corruption is one potential explanation, but it is unlikely that local politicians can appropriate an entire budget expansion without leaving any observable change. By allowing responses to vary with the size of the rents, I show that the relationships between mining transfers, several public goods, and household income change across the transfer distribution. Finally, the private and decentralized organization of mineral production in Peru limits the ability of any single local government to

determine production or fiscal revenues. I use subnational variation and panel fixed effects to account for persistent differences in local economic and political institutions.

More broadly, the paper contributes to the longstanding debate about the political consequences of resource abundance and their implications for citizens' living standards. A growing literature uses subnational variation to study how resource booms affect civil conflict, corruption, political participation, confidence in political institutions, and local-government efficiency.[8] Other studies examine public-good provision and living standards (Olsson and Valsecchi, 2015; Caselli and Michaels, 2013; Monteiro and Ferraz, 2012; Pebe et al., 2017; Loayza and Rigolini, 2016; Zambrano et al., 2014), local-input demand and market-level externalities (Allcott and Keniston, 2018; Aragon and Rud, 2013), the interaction between royalties and other revenue sources (Martínez, 2025), and the role of institutional reforms in preventing the resource curse (Gallego et al., 2020). I complement this work by showing that the marginal relationship between a fiscal windfall and local political and economic outcomes can change substantially across the transfer distribution.

The remainder of the paper is organized as follows. Section 2 provides some background details about the institutional setting. Section 3 introduces a basic conceptual framework and Section 4 presents the empirical strategy. Section 5 describes the data and Section 6 outlines the empirical results. Section 7 places the results in context with the existing literature and Section 8 offers some conclusions.

2 Institutional Background

2.1 Local politics

Peru's subnational organization distinguishes territorial jurisdictions from governing institutions. The country is divided into regions, which contain provinces, which in turn contain districts. Regional governments operate at the regional level, while provincial and district municipalities constitute the two types of local government. A provincial municipality governs the capital district of its province and also performs province-wide functions. Throughout the paper, *district* denotes the territorial jurisdiction and *municipality* or *local government* denotes its governing institution. I use *province* and *region* for the larger geographic areas relevant to the statutory allocation rule and the sample restrictions.

Local governments are relatively recent as independent political units in Peru.[9] The 1979 Constitution was the first to recognize their political autonomy.[10] Under the current legal framework,

[8] Research on conflict includes Angrist and Kugler (2008), Dube and Vargas (2013), and Arellano-Yanguas (2011); studies of corruption include Brollo et al. (2013), Maldonado (2011), and Vicente (2010). Related work examines political participation and confidence in political institutions (Armand et al., 2020; Maldonado, 2012) and local-government efficiency (Borge et al., 2015; Maldonado and Ardanaz, 2023).

[9] During the period covered by the original administrative sources, the municipal system comprised 1,645 district municipalities and 195 provincial municipalities. The empirical analysis uses a harmonized geography of 1,792 district jurisdictions. Because a provincial municipality also governs its capital district, provincial and district municipalities are not stacked as separate observations for that district.

[10] Direct local elections were first introduced in 1963 during Fernando Belaunde Terry's first presidential term. Mu-

each municipality is governed by a mayor and a municipal council. Electoral rules give the mayor substantial discretion: irrespective of the vote margin, the winning list receives half of the council seats plus one. This institutional arrangement limits the ability of opposition parties to hold the mayor accountable through the municipal council.

Elections for mayor and municipal council are held every four years. During the period covered by the analysis, mayors could seek immediate reelection, although the recall mechanisms introduced by the 1993 Constitution allowed citizens to remove local authorities before the end of their terms.[11] The possibility of recall may shorten politicians' effective horizons.[12] Immediate mayoral reelection was still permitted in the October 2014 municipal election, yet the Congress was debating many reform proposals to prohibit it since 2013.[13] I therefore exclude that electoral cycle to keep the incentive to seek reelection comparable across the elections analyzed, without assuming that anticipatory behavior is established.

Following the collapse of the national party system, national political parties have played a limited role in local elections. In a survey conducted for a World Bank study, local politicians reported receiving no funding from national parties and displayed little evidence of party loyalty (World Bank, 2001). Indeed, mayors frequently seek reelection under a different political label. Local politics has also become increasingly fragmented with the growth of provincial and local political movements that have weak links to national parties.[14] As a result, local politics is highly personalized and increasingly disconnected from national politics.[15]

The legal framework assigns local governments both exclusive and shared responsibilities. Ex-

nicipal elections were suspended after General Juan Velasco Alvarado overthrew Belaunde in October 1968. Between 1968 and the return to civilian rule in 1980, the executive branch—most commonly the Ministry of the Interior—appointed local mayors.

[11] Law 27734, enacted in May 2002, amended several provisions of the Municipal Elections Act (Law 26864); before this reform, municipal elections were held every three years. The electoral analysis ends in 2010. By the 2014 election, proposals to prohibit immediate reelection had been pending in Congress since 2011 and the reform was under active plenary debate. Congress approved the reform for the first time on October 23, 2014, and it became Law 30305 in March 2015. Although the new rule did not formally govern the 2014 contest, the reform process could have affected the entry and reelection decisions of incumbents and challengers. Including that election would therefore make the no-anticipation condition underlying the panel interpretation less credible.

[12] Anecdotal and empirical evidence reported by Artiles et al. (2019) suggests that recall elections have been used to remove mayors in mineral-rich areas. This evidence makes the political-competition channel discussed in Section 3 especially salient.

[13] The congressional legislative record lists Bill No. 2814/2013-CR, submitted on October 22, 2013, among the proposals later consolidated into a reform prohibiting it. Congress debated the reform in June and August 2014, gave it a first approval on October 23, 2014, after the municipal election, and approved it again on March 5, 2015. A survey found that 72% approved prohibiting the immediate reelection. Constitutional Reform Law No. 30305, published on March 10, 2015, amended Article 194 to prohibit immediate mayoral reelection. The pending reform could have changed incumbents' expectations during the 2011–2014 term, even though the prohibition was not yet in force.

[14] According to Oficina Nacional de Procesos Electorales (2010), 72% of the candidates in the 2002 municipal elections belonged to provincial or local political organizations.

[15] This political autonomy coexists with severe administrative constraints. Using the 2011 National Municipal Registry (RENAMU), I estimate that only 14% of local governments had urban-development plans, 20% had local-development plans, and 29% had cadastral information systems. Municipalities also lacked stable and qualified personnel: only 21% of local public employees held permanent contracts, 50% held temporary contracts, and 19% had professional degrees. These limitations help explain the low investment capacity of many municipalities.

clusive functions include urban and rural development, the regulation of local public services, municipal organization and planning, and the execution and monitoring of local infrastructure (World Bank, 2012). Shared functions require coordination with provincial, regional, or central authorities.[16] This division of responsibilities is important for interpreting the public-good results because municipalities do not control every service observed in their jurisdictions.

2.2 Local public finance

Peru remains highly centralized from a fiscal perspective: the central government collects 97% of all taxes, while municipalities have little authority to create their own taxes. Property taxes on vehicles, real estate, and real-estate transfers accounted for 90% of municipal tax revenue in 2007, leaving only a marginal role for taxes on production and consumption. Even so, own-source tax revenue represented no more than 13% of total municipal income (World Bank, 2012; Canavire-Bacarreza et al., 2012).

Consequently, local governments depend heavily on transfers from the central government. In 2008, these transfers represented 75% of municipal budgets, and the Fondo de Compensación Municipal (FONCOMUN) accounted for 33% of intergovernmental transfers (Canavire-Bacarreza et al., 2012). FONCOMUN is distributed to every municipality, whereas targeted transfers include canon categories associated with oil, mining, hydropower, forests, gas, and fishing. These categories describe the broader municipal fiscal system, but the treatment used in this paper includes only Mining Canon and mining royalties. Mining Canon was the largest canon category. Together with mining royalties, it represented 29% of local-government budgets nationally and close to 70% in some producing districts (Canavire-Bacarreza et al., 2012).

2.3 Mining sector in Peru: A brief overview

Mineral production increased substantially after the government introduced pro-market policies to attract foreign investment during the 1990s. Figure 1 presents the evolution of the real value of production from 1996 through 2010. Production more than doubled between 2000 and 2010, and much of this expansion occurred before international mineral prices began rising broadly in 2003. This timing is consistent with the new regulatory framework contributing to the increase in production.[17]

Although production contributed to the mineral boom, the increase in international prices was more pronounced. Figure 2 presents the evolution of copper, gold, silver, and zinc prices between 1996 and 2010.[18] Prices were comparatively stable through 2003 and then rose sharply,

[16] These responsibilities include school services, public health, culture, sports and recreation, citizen security, transport, housing, social programs, and waste management. The overlap has generated coordination problems across levels of government and has weakened the implementation of some economic-development and social programs.

[17] Section A.1 of the Online Appendix provides additional details on the regulatory changes introduced during the 1990s.

[18] Online Appendix Figure A.1 reports the corresponding series for the other minerals produced in Peru.

although their trajectories diverged after the global financial crisis. The variation in production remains relevant because extraction may affect local outcomes independently of the fiscal transfers generated by the boom.

The central statutory change preceded the mineral-price boom. Law 27506, enacted in July 2001 as part of the decentralization process, raised the subnational share of mining income-tax revenue from 20% to 50%. The new rule took effect in 2002, while international mineral prices began their broad increase only after 2003 (Figure 2). The initial increase in the subnational share was therefore not adopted in response to the realized boom. Laws 28077 (September 2003) and 28322 (August 2004) subsequently refined the allocation within eligible areas without changing the aggregate 50% share (Barrantes et al., 2010). Under the resulting rule, 10% goes to the municipality governing the producing district, 25% to municipalities in the producing province, 40% to municipalities in the producing region, and 25% to the regional government; one fifth of the regional share is reserved for public universities. These amendments changed the distribution across eligible jurisdictions, not the aggregate fiscal shock generated by subsequent prices and taxable mining income.[19]

The two components of the treatment are fiscally distinct. Mining Canon redistributes to subnational governments a share of the corporate income tax paid by mining companies, whereas mining royalties are payments tied directly to the exploitation of mineral resources. In the administrative data, total mining transfers equal Mining Canon plus mining royalties; Oil Canon and the other canon categories are excluded.[20]

Figure 3 describes the evolution of total mining transfers. The combined amount of Mining Canon and mining royalties was approximately 89 million real PEN in 2001 and 126 million in 2002, before reaching 3.95 billion in 2007. As a point of reference, the average exchange rate over the period was 2.85 PEN per US dollar. The total subsequently declined to 2.49 billion real PEN in 2010. This windfall disproportionately benefited producing districts and created substantial inequalities across municipalities.[21]

The expansion of mining transfers was also geographically uneven because mineral extraction and the statutory redistribution of its fiscal proceeds were concentrated in producing regions. Figure 4 maps the change in real mining transfers per capita between the 1996–1998 and 2008–2010 district averages. Using three-year windows reduces the influence of idiosyncratic values in either endpoint year. The largest increases occurred in southern Peru, particularly in Tacna, Moquegua, and Cusco,

[19] Section A.2 of the Online Appendix describes the allocation rules in greater detail, and Online Appendix Table A1 summarizes them. Arellano-Yanguas (2011) argues that this localist framework intensified conflict because national institutions offered limited support to local governments, firms, and communities negotiating the consequences of extraction. The fiscal windfall therefore operated within a politically contested institutional environment.

[20] Online Appendix Table A2 reports the composition of mining transfers in election years. Mining Canon accounts for 100% of the treatment in 1998 and 2002, 80.8% in 2006, and 83.0% in 2010; royalties account for the remaining 19.2% and 17.0% in the last two elections. The fiscal records aggregate municipal receipts across eligible mining activity and do not identify the mineral that generated each payment. Mineral-specific quantities and values enter separately in the production data and controls.

[21] Online Appendix Figure A.2 presents mining transfers by district type and shows that producing districts received the largest amounts. The Gini coefficient for average real mining transfers per capita over 1996–2010 is 0.767; Online Appendix Figure A.3 presents the corresponding Lorenz curve.

as well as in Ancash and Cajamarca. By contrast, large parts of the Amazon and the northern coast experienced small or no increases. The map describes the spatial incidence of the transfer expansion rather than an estimated causal effect.[22]

3 Conceptual framework

An extensive theoretical literature examines the political consequences of resource booms. I organize the empirical analysis around the model developed by Caselli (2006), which brings together several mechanisms emphasized in previous work on the political resource curse. The model considers a two-period local economy with a large number of unskilled workers and two talented agents: an incumbent and a potential challenger. In the first period, the economy has mineral-resource and subsistence sectors, and the mineral sector provides an exogenous flow of rents to the local government.[23] Production in the local economy also depends on the public goods provided by the government. The incumbent can therefore affect local output through the allocation of rents between personal consumption and public-good provision.

The potential challenger decides whether to become an industrialist in the second period or to compete against the incumbent. This decision compares the net benefit of political office with the opportunity cost of entering the industrial sector. The incumbent, in turn, maximizes the present value of consumption while accounting for how public-good provision affects the talented agent's occupational choice. Greater incumbent consumption and lower public-good provision reduce the returns to industrial activity and make political entry more attractive. Conversely, greater public-good provision raises the opportunity cost of challenging the incumbent.

The timing of the game is straightforward. In the first period, mineral rents, public goods, and the identity of the incumbent are given. At the end of that period, the incumbent chooses consumption and public-good provision for the second period. After observing that choice, the talented agent decides whether to enter the industrial sector or challenge the incumbent. An election takes place at the end of the second period.

The solution produces a non-monotonic relationship between mineral rents and political competition. An increase in per capita rents raises the value of holding office and makes politics more attractive to the talented agent. At the same time, the incumbent has additional resources that can be used to provide public goods, raise the returns to industrial activity, and discourage political entry. At low rent levels, this strategy may allow the incumbent to prevent the talented agent from entering politics. At sufficiently high rent levels, however, the value of office exceeds the profits available in the industrial sector, and additional public-good provision can no longer deter entry.

[22] Online Appendix Figure A.4 maps districts with positive physical mineral production in at least one year between 1996 and 2010. Producing districts constitute only a small fraction of all districts receiving Mining Canon transfers.

[23] This formulation isolates the fiscal "rent effect" and does not incorporate the direct local consequences of mineral extraction. Modeling the "production effect" is more difficult because higher production may generate both positive and negative externalities. The empirical analysis therefore controls for production and separately examines non-producing recipients.

Beyond a threshold level of rents, denoted by c^*, the incumbent reduces public-good provision because the probability of remaining in office has fallen.

For the municipal application, I use a reduced-form electoral analogue of the model's counter-challenge technology. The incumbent may allocate fiscal resources to expenditures that reduce a challenger's probability of winning, including clientelistic spending. The effectiveness of this strategy determines how the two incentives vary with the size of the rents. When the strategy is ineffective, rents primarily increase the value of obtaining office. When it is moderately effective, the dominant force may change as rents increase. When it is highly effective, the incumbent can protect the position over a wider range of rents. The framework therefore permits non-monotonic political responses, but it does not assign a unique shape to every electoral outcome.

The mapping from this framework to the empirical analysis is deliberately limited. Caselli's original model concerns broader struggles for power and investment in development rather than municipal elections or the distribution of candidate vote shares. I use the model to organize the two countervailing incentives generated by a resource windfall, not as a structural representation of the Peruvian electoral process. The theoretical outcome that maps most directly into the data is the incumbent's probability of remaining in office; vote-share dispersion and candidate entry are related electoral outcomes, but neither provides a direct test of the challenger type in the model.

The model does not, however, derive a quadratic response. I use the empirical quadratic as a parsimonious approximation that allows the marginal relationship to change with the size of the transfer while imposing constant curvature and at most one turning point. When $\beta_1 < 0$ and $\beta_2 > 0$, the estimated transfer component is convex and U-shaped over its full mathematical domain. A reversal need not characterize most districts to be substantively important: exceptionally large rents may be precisely where political incentives change most. Whether the data identify such a reversal, however, depends on support and uncertainty in that part of the distribution. This distinction guides the graphical and inferential analysis below.[24]

4 Empirical Strategy

4.1 Identification

The mineral-price boom raised both mineral production and mining transfers. My identifying argument does not assume that realized transfers are mechanically exogenous. These transfers combine international prices, local production and taxable profits, and statutory allocation rules. International price movements are plausibly external to the political choices of an individual Peruvian municipality, whereas the production and allocation components require separate controls and robustness checks. Distinguishing the fiscal effect of transfers from the local effect of extraction

[24]The quadratic specification summarizes changing slopes; it should not be interpreted as a structural functional form implied by the model. I evaluate its adequacy against higher-order polynomials and a semiparametric fixed-effects estimator in Section 6.3.

is difficult because both respond to the boom and need not affect political outcomes in the same direction.[25]

Peru is an important mineral producer, and I do not assume that its aggregate supply is irrelevant to world mineral markets. In 2010, the country accounted for approximately 6.8% of world copper mine production and 13.7% of world tin mine production, two minerals for which its international rank was especially high (Wilburn, 2013). These shares are economically meaningful but remain a minority of global output. More importantly for identification, production was distributed across independently operated mines rather than controlled by municipal governments. The relevant assumption is that no individual district government in the sample can set the benchmark prices used in mineral transactions.[26]

The timing and breadth of the 2000s commodity boom also point to substantial external demand forces. The World Bank attributes the early part of the metals boom to demand growth that outpaced supply capacity, with China's increasing metal intensity playing a central role (World Bank, 2009). Time-series evidence likewise indicates that shocks to Chinese economic activity had persistent effects on several base-metal prices (Roache, 2012).[27] This evidence supports the external origin of the price shock faced by Peruvian municipalities, but it does not establish the exogeneity of realized district transfers. Even if a Peruvian supply response dampened world prices, that mechanism would reduce the common price shock without assigning a unique sign to the bias in the political-outcome regressions. I therefore distinguish transfers from local extraction rather than relying on a categorical price-taker assumption.

Another concern is reverse causality between local politics and mineral production. Although municipal governments do not grant mining rights or environmental permits, local authorities in producing districts may influence the social conditions under which existing mines operate. They may mobilize residents to demand additional resources or, conversely, help contain protests in exchange for employment or other local benefits. Production may therefore respond to both international prices and local political incentives. Conditioning on contemporaneous production does not eliminate this concern because production may be jointly determined with political outcomes or may itself respond to the price boom.[28]

I address this problem by exploiting the geography of the transfer rule. Using the annual

[25] Higher transfers may allow an incumbent to expand public-good provision. Additional extraction may also raise local income and employment or, conversely, generate pollution and conflict. The sign of omitted-production bias is therefore not determined a priori.

[26] Peru has historically been a small open economy that depends heavily on primary exports and is therefore sensitive to external shocks. Dancourt (1999), for example, attributes a substantial part of the country's post-1950 economic fluctuations to changes in the terms of trade. This aggregate sensitivity does not imply that a municipality can determine international mineral prices.

[27] According to the evidence summarized by Winters and Yusuf (2007), China accounted for 24% of world mineral consumption and absorbed approximately two thirds of the increase in global metal production between 1999 and 2005. Falling interest rates provide another potential explanation for the commodity-price boom (Frankel, 2008).

[28] International prices may also affect producing districts through corporate social-responsibility programs or local suppliers. Such channels reinforce the distinction between the fiscal-transfer and production effects. They are much less plausible in non-producing districts, which motivates the restricted-sample analysis below.

production panel for 1996–2010, I classify a district as a producer if it records positive physical mineral production in any sample year. I remove the 159 producing districts for their entire observed history rather than only in years with positive production. I then examine never-producing districts located in producing regions or provinces. Their municipalities receive mining transfers through the statutory allocation formula but do not host the extraction activity that a local mayor could influence. This restriction does not eliminate every possible regional spillover, but it removes the direct district-level politics–production–transfer mechanism described above.

A final concern is the political economy of the allocation rule. The chronology limits, but does not eliminate, this problem. The principal reform was enacted in 2001, before the international price boom, as part of the broader decentralization process. The 2003 and 2004 amendments preserved the 50% subnational share while changing its allocation within producing areas. These amendments may reflect political bargaining among representatives of mineral-rich regions, and I therefore do not treat the distribution formula as apolitical.[29] At the same time, the rule was national and statutory rather than negotiated separately by each district mayor, and year fixed effects absorb its common timing. The amendments may therefore affect the geographic incidence of the treatment without implying that the electoral choices of individual incumbents generated the transfer boom.

The central government may also reduce other transfers when mining transfers increase. Such fiscal substitution is one mechanism through which the boom changes a municipality's net resources; it is not, by itself, a confounder. The baseline coefficient captures the total political response to the observed mining-transfer shock, including any formula-driven offset in other revenues. Conditioning on a transfer that responds mechanically to mining revenue would block part of this response and recover a controlled direct effect instead.[30]

4.2 Empirical model

My main empirical design is a panel fixed-effects model with a continuous treatment. I use within-district changes in mining transfers during the mineral-price boom, net of common year shocks and time-invariant district characteristics. This is not a conventional binary difference-in-differences design because treatment intensity varies continuously across districts and election cycles. The causal interpretation therefore requires that, conditional on the stated controls and fixed effects, no time-varying district shock jointly determines mining transfers and the outcomes.[31]

For district-level outcomes, the estimating equation is

[29] Barrantes et al. (2010) argue that changes in the Mining Canon law resulted from circumstantial alliances among legislators from mineral-rich regions rather than pressure by local authorities or the executive branch. This evidence reduces, but does not eliminate, concerns that the rule responded to local political outcomes.

[30] Selective reductions based on unobserved mayoral electoral prospects would create a different identification problem. I examine FONCOMUN, the principal universal transfer, in the robustness analysis below.

[31] This distinction also limits the relevance of concerns developed for staggered binary adoption with heterogeneous treatment effects. The coefficients here summarize a functional relationship in a continuously treated panel; they are not weighted averages of cohort-specific binary treatment effects. The maintained restriction is instead the conditional parallel-trends assumption for changes in treatment intensity.

$$y_{jt} = \alpha_j + \varphi_t + \beta_1 R_{jt} + \beta_2 R_{jt}^2 + \gamma Q_{jt} + \epsilon_{jt}, \tag{1}$$

whereas household-level outcomes are estimated using

$$y_{ijt} = \alpha_j + \varphi_t + \beta_1 R_{jt} + \beta_2 R_{jt}^2 + \gamma Q_{jt} + X_{ijt}^{'}\delta + \epsilon_{ijt}. \tag{2}$$

In the first equation, y_{jt} is a political, fiscal, public-good, or municipal-employment outcome observed for district j in period t. Political outcomes are observed in election years and use either transfers received in the election year or average transfers over the preceding electoral cycle; annual administrative outcomes retain their original timing. In the second equation, y_{ijt} is an outcome for household or individual i in district j and year t. ENAHO consists of repeated cross-sections rather than a household panel. Different households may be sampled each year, so the model includes district and year fixed effects but not household fixed effects.

The terms α_j and φ_t denote district and year fixed effects. The treatment R_{jt} is real mining transfers per capita, measured in thousands of 2001 PEN. In the preferred specifications, Q_{jt} is the logarithm of one plus the real value of mineral production in district j.[32] Household and individual covariates are collected in X_{ijt}. I cluster standard errors at the district level because treatment and the fixed-effect identifying variation occur at that level. The coefficients β_1 and β_2 describe the conditional within-district relationship between political outcomes and mining transfers per capita. The main objects of interest are the marginal effect $\partial y/\partial R = \beta_1 + 2\beta_2 R$ and the implied turning point $-\beta_1/(2\beta_2)$. I interpret both quantities over the observed support of transfers and report their statistical uncertainty.

The quadratic specification is motivated by the changing incentives in the conceptual framework, but it is not derived structurally from that model. It imposes constant curvature and at most one turning point, and I evaluate its adequacy in the robustness analysis. Throughout the paper, I distinguish average transfers over the electoral cycle from transfers received in the election year. I do not combine results based on these two definitions in a single curve.

Year fixed effects absorb shocks common to all districts, while district fixed effects absorb time-invariant local characteristics. The remaining variation comes from differential within-district changes in transfer intensity. The main threat is a district-specific, time-varying shock that affects both transfers and political outcomes. Mineral production is one such channel: it may confound the fiscal effect, but it may also respond to the price boom and therefore constitute a post-treatment variable.[33]

I therefore treat production controls and sample restrictions as complementary rather than

[32] Production enters linearly because the conceptual framework motivates nonlinearity in fiscal rents, not in mineral output. I examine alternative transformations of production below.

[33] In this sense, contemporaneous production may be a "bad control" (Angrist and Pischke, 2009). Its inclusion requires a conditional-mean assumption and should not be interpreted as making production exogenous. For this reason, I treat the results for non-producing recipients as a separate and more direct way to isolate the fiscal-transfer channel.

interchangeable. The all-district models compare estimates with and without contemporaneous production, while the never-producer estimates provide the most direct separation between the fiscal and local-extraction channels. I determine producer status from the mineral-specific physical quantities in the annual production panel.[34]

Finally, I report a secondary IV sensitivity exercise that uses Mining Canon and its square as instruments for total mining transfers and their square. I use the imperfect-instrument bounds proposed by Nevo and Rosen (2012) to examine how the estimates change when the instrument is allowed to be correlated with the structural error.[35]

I conclude this section with a discussion of inference. Since Moulton (1986), researchers have recognized that ignoring within-group dependence can severely understate standard errors. Serial correlation creates an additional problem in panel settings (Bertrand et al., 2004). I therefore use the generalization of the White (1980) covariance matrix developed by Liang and Zeger (1986) and cluster standard errors at the district level, where treatment and the identifying within-unit variation are defined. The four preferred all-district and never-producer political models contain between 1,449 and 1,595 district clusters.[36]

The statutory allocation rule may also correlate shocks across districts in the same province, so I evaluate province-level clustering in the robustness analysis. I do not cluster by election year because the political models contain only three post-baseline elections, too few to justify cluster-robust inference in the temporal dimension.[37]

5 Data

5.1 Data sources

The empirical analysis uses a unique dataset that combines electoral outcomes, intergovernmental transfers, public-good provision, local-government characteristics, and measures of living standards for 1996–2010. Electoral outcomes come from the Oficina Nacional de Procesos Electorales (ONPE), Peru's electoral authority. I assemble a district-election panel for the ordinary municipal elections held in 1998, 2002, 2006, and 2010 and use it to construct the two main politi-

[34] A price-based prediction of transfers would require assigning corporate profits and income-tax payments to individual districts and replicating the Ministry of Economy and Finance allocation formula for non-producing municipalities. Tax authorities treat firm-level profit information as confidential, firms often operate in more than one district, and the allocation to non-producing districts depends on population and poverty formulas. These limitations prevent the construction of a credible district-level predicted-transfer series that reproduces the treatment used here.

[35] This exercise is motivated by the external component of the mineral-price shock but does not eliminate the concerns associated with IV estimation. Young (2022) emphasizes that conventional IV estimates may be imprecise and, in some settings, little different from biased least-squares estimates. I therefore report first-stage diagnostics and treat the IV results as sensitivity evidence rather than as the principal design.

[36] Cluster-robust inference relies on a sufficiently large number of independent clusters. This requirement is well satisfied at the district level. For a discussion of inference with few clusters and bootstrap-based alternatives, see Angrist and Pischke (2009) and Cameron et al. (2008).

[37] Election fixed effects absorb shocks common to all districts in each election year. They do not, however, substitute for a sufficiently large number of temporal clusters.

cal outcomes: mayoral reelection and political competition.

Data on municipal revenues and mining transfers for 2001–2010 come from the Ministry of Economy and Finance. These records include transfers received from the central government and regular municipal revenues such as taxes, contributions, and service fees. I harmonize the fiscal data to a fixed 1998 geography of 1,792 district jurisdictions and use them to examine how politicians allocate municipal budgets. I extend the Mining Canon series backward using Ministry of Energy and Mines records for 1996–2000. This division reflects the coverage of the two administrative sources rather than the legal availability of Mining Canon: the transfer existed under the 1992 framework, the 2001 Canon Law increased the subnational share, and the Ministry of Economy and Finance provides the 2001–2010 observations in the combined series.

I construct a panel of local-government characteristics from the National Municipal Registry (RENAMU), a census of municipalities administered by the National Statistical Institute. It contains information on municipal personnel, assets, public-good provision, budgets, and district socioeconomic characteristics. The harmonized variables cover 2001–2010, subject to the availability of each outcome.

Information on mineral prices and production comes from the Ministry of Energy and Mines and covers 1996–2010. I use these data to value district mineral production at fixed 2001 mineral prices.

I obtain additional information from the National Household Survey (ENAHO), an annual nationally representative survey administered by Peru's statistical agency. ENAHO contains detailed information on household living conditions. I use repeated cross-sections from 1998 through 2010 to examine how the fiscal boom is associated with household income and consumption.

Finally, I use the 1993 Census of Population and Housing to compare pre-treatment district characteristics across levels of mineral production and mining transfers. Annual district population estimates come from INEI.

I make two adjustments to account for inflation and changes in district boundaries during the study period. First, I convert nominal values into December 2001 prices and then apply a spatial deflator that expresses real values in metropolitan Lima prices for that year. Second, I construct homogeneous geographic identifiers for 1993–2010 and map every source to the fixed 1998 district geography used in the empirical analysis.[38]

5.2 Units of observation and sample construction

The unit of observation varies with the outcome. The electoral analysis uses a district-election panel for 1998, 2002, 2006, and 2010. Reelection is defined for 2002, 2006, and 2010 because it links the winner of the preceding election to the current outcome; the political-competition regressions use the same three post-baseline elections to preserve common timing. Transfers, mineral production,

[38]The geographic harmonization preserves correspondence across administrative sources when districts split or their identifiers change. Without this step, a new district could be incorrectly treated as a different economic unit before and after the boundary change.

municipal budgets, employment, and public-good outcomes are observed at the district-year level, while the 1993 Census characteristics are measured once at the district level. The ENAHO sample consists of household-year observations linked to district-year fiscal exposure and does not follow the same households over time. I do not pool provincial or regional governments with district observations. Specifications restricted to producing provinces or regions retain the district as the unit of observation and change only the set of districts included in the estimation.

The harmonized geographic spine contains 1,792 district jurisdictions in each of the four election years, yielding 7,168 unique district-election cells. The geographic spine is balanced, but the regression panels are not because electoral outcomes and cycle-average transfers are unavailable for some district-elections. Tables 2 and 3 therefore report both complete-case observations and distinct districts for each panel and specification.[39]

5.3 Main variables

I construct the main dependent variables from ONPE electoral data. Following the conceptual framework, the two political outcomes capture incumbency advantage and political competition. For the first outcome, I define an indicator equal to one if the incumbent mayor is reelected.[40] Political competition is multidimensional: candidate counts capture nominal entry, turnout captures citizens' participation, and vote margins capture the closeness of the leading candidates. These dimensions need not move together.[41] Following Skilling and Zeckhauser (2002), I use one minus the Herfindahl–Hirschman index as the main measure because it incorporates the complete distribution of candidate vote shares.[42]

The treatment variable is total real mining transfers. This fiscal measure is not constructed by summing the production of individual minerals, and the revenue records do not decompose receipts by copper, gold, silver, zinc, or other minerals.[43] I express transfers in 2001 Lima prices and convert them to thousands of PEN per capita using annual district population estimates. Two timings capture different political margins. Election-year transfers measure the resources available closest to the vote and constitute the preferred timing for mayoral reelection. Average transfers over the preceding electoral cycle measure the cumulative fiscal environment during the mayoral term and constitute the preferred timing for vote-share dispersion and candidate entry. Tables 2 and 3 report both measures for both outcomes.

[39] Online Appendix Table A3 reports the sample flow and observation counts by election year.

[40] This is a measure of individual rather than party reelection. As discussed in Section 2, the electoral arena is highly fragmented and political parties are weak. A party-based measure would therefore fail to capture the underlying political process because politicians frequently change party labels between elections.

[41] Candidate counts alone can overstate effective competition: several weak candidates may enter while votes remain concentrated among one or two leading contenders. I examine candidate entry as a complementary outcome below.

[42] See Bardhan and Yang (2004) for a conceptual discussion of alternative measures of political competition. The Herfindahl–Hirschman index is $H = \sum_i s_i^2$, where s_i denotes candidate i's vote share; values of $PC = 1 - H$ closer to one indicate greater vote-share dispersion.

[43] Section A of the Online Appendix describes the composition and allocation of mining transfers in greater detail.

Tables 7–10 report the corresponding estimates for public-good provision, municipal employment, local-government expenditures, and household living standards.

5.4 Summary statistics

Table 1 reports summary statistics by exposure group using district-year information from 1998 through 2010. The table distinguishes non-producing recipients, producing district-years, and district-years that receive no mining transfers. It describes political outcomes, mining transfers, municipal budgets, mineral production, and predetermined district characteristics from the 1993 Census. These comparisons document the substantial differences in exposure generated by the allocation system; they are not estimates of the effects of mining transfers.

The transfer distribution is highly right-skewed. In the reelection sample, election-year transfers average 0.129 thousand real PEN per capita, with a standard deviation of 0.552. The median, however, is only 0.015; the 90th and 99th percentiles are 0.290 and 1.776. Average transfers over the electoral cycle in the political-competition sample have a mean of 0.127, a median of 0.009, and a 99th percentile of 1.818. The standard deviation is therefore informative about dispersion but does not represent a typical treatment change. This distinction is especially important in the quadratic specification, where the marginal effect depends on the initial transfer level. I consequently interpret marginal effects at observed transfer levels and report the upper percentiles of the treatment distribution.

The reelection rate varies between 11% and 19% across the three exposure groups, while the political-competition index ranges from 0.81 to 0.84. Differences in fiscal exposure are considerably larger: average real mining transfers per capita equal 105.34 PEN among non-producing recipients and 553.97 PEN in producing district-years.[44]

6 Empirical Results

6.1 Reelection outcomes

Table 2 presents the relationship between mining transfers and mayoral reelection using the panel fixed-effects model. In the upper panel, the treatment is average per capita mining transfers, measured in thousands of PEN at 2001 Lima prices, over the electoral cycles 1998–2002, 2002–2006, and 2006–2010. The lower panel instead uses transfers received in the election years 2002, 2006, and 2010.[45] The dependent variable is an unconditional office-retention indicator equal to one if the mayor elected in the preceding contest wins the office again and zero otherwise. A zero therefore

[44] Online Appendix Table A4 reports complete descriptive statistics for the preferred political estimation samples. Online Appendix Tables A5–A7 provide the corresponding statistics for the public-good, municipal-employment, expenditure, and household samples.

[45] The two timings distinguish cumulative fiscal resources over a mayoral term from resources available closest to the election, as emphasized by the political-budget-cycle literature (Nordhaus, 1975).

includes incumbents who do not seek another term and those who run but lose. Conditioning reelection on candidacy would select on a decision that may itself respond to transfers.[46]

Column 1 presents the linear specification. An additional 1,000 PEN per capita in average mining transfers is associated with a 2.6 percentage-point reduction in the probability of reelection. The estimate is statistically significant at the one-percent level and represents approximately 14% of the sample mean of 18.3%. By contrast, the linear estimate for election-year transfers is small and not statistically distinguishable from zero.

In column 2, I introduce the quadratic specification motivated by the conceptual framework. Neither coefficient for average transfers is statistically distinguishable from zero. For election-year transfers, however, the level coefficient is negative (-0.065) and the quadratic coefficient is positive (0.007); both are statistically significant at the one-percent level. The relationship between transfers and reelection therefore depends on the amount received by the district. Figure 5 presents the corresponding marginal effects and support diagnostics.

Column 3 controls for the logarithm of the real value of mineral production in the district. The production coefficients are close to zero and not statistically significant, while the transfer estimates remain close to those in column 2. This pattern is consistent with the descriptive evidence that prices, rather than a contemporaneous expansion of physical output, explain much of the increase in mining transfers. The alternative production measures and results among never-producing recipients below show that the transfer pattern persists without relying on contemporaneous changes in local production.

Online Appendix Table A9 examines whether this conclusion depends on the scale or transformation of production. I hold the estimation samples fixed and compare the baseline logarithmic control with the logarithm, inverse hyperbolic sine, and level of real production per capita. The sign and statistical precision of both transfer terms remain stable across these specifications. I retain the logarithm of total real production as the baseline because it compresses the extreme scale differences across producing districts; I report the per capita alternatives as robustness checks.[47]

Columns 4–6 reproduce the quadratic specification with production controls in three restricted samples. Column 4 excludes Lima and Callao because Lima accounts for more than half of the country's GDP and differs substantially from the rest of the country. This restriction has little effect on the election-year estimates: the coefficient on mining transfers is -0.060 and the coefficient on its square is 0.007. The average-transfer coefficients also remain close to the all-district estimates and are not statistically distinguishable from zero.

[46] Online Appendix Table A8 decomposes the complete incumbent population into three mutually exclusive outcomes: does not run, runs and loses, and runs and wins. I link incumbents to candidate rosters using national identification numbers when available and normalized names within district otherwise.

[47] For reelection, the linear transfer coefficient ranges from -0.062 to -0.068, the quadratic coefficient remains close to 0.007, and the implied turning point ranges from 4.48 to 4.75 thousand PEN per capita. For political competition, the corresponding coefficients range from -1.010 to -1.206 and from 0.044 to 0.050. The production coefficient is precisely estimated only when entered in levels in the political-competition model, but the transfer estimates remain stable. Because physical output is recorded in mineral-specific units rather than a common aggregate in tonnes, the non-producer specifications also avoid imposing arbitrary conversion weights.

Column 5 retains districts located in producing regions. Districts in regions without mineral production may differ systematically from those in producing regions and therefore provide a less informative counterfactual. This restriction compares districts exposed to the statutory transfer system that differ primarily in the intensity of the fiscal windfall. The level and quadratic coefficients remain similar in magnitude and statistical precision, indicating that districts outside producing regions do not drive the main pattern.

Column 6 applies the same logic within producing provinces, a substantially narrower geographic comparison. Again, the coefficients remain similar in magnitude and statistical precision.

The contrast between the two treatment timings is informative. The election-year results remain stable across specifications, whereas the estimates based on average transfers over the electoral cycle are statistically distinguishable from zero only in the linear model. This pattern is consistent with evidence that intergovernmental transfers are especially responsive around elections.[48] For Peru, Pebe et al. (2017) find that capital spending responds differentially among the 20% of districts receiving the largest Canon amounts, although they find no general political budget cycle. My election-year estimates concern mayoral reelection rather than capital-spending cyclicality and therefore capture a distinct electoral margin.

The unconditional decomposition in Online Appendix Table A8 clarifies the electoral margin behind this result. At low transfer levels, the reduction in reelection is accompanied primarily by a higher probability that the incumbent runs and loses rather than leaves the contest, although the two component estimates are individually imprecise. None of the corresponding coefficients based on average transfers over the electoral cycle is statistically distinguishable from zero.[49]

Figure 5 connects the quadratic coefficients to the estimated relationship and its slope. The upper panels plot the transfer component $\widehat{g}(R) = \widehat{\beta}_1 R + \widehat{\beta}_2 R^2$ relative to zero transfers. The middle panels report the marginal effect $\widehat{\beta}_1 + 2\widehat{\beta}_2 R$ and its pointwise 95% confidence interval, calculated from the joint district-clustered covariance matrix of the linear and quadratic coefficients. The lower panels show the treatment distributions. The left column uses election-year transfers for reelection and ends at the 99th percentile, or 1.776 thousand real PEN per capita. At the sample mean of 129 PEN per capita, the estimated marginal effect is -0.062, with a 95% confidence interval from -0.110 to -0.015. The point estimate remains negative through the 99th percentile, where it equals -0.039 and its confidence interval remains below zero.

The coefficient estimates imply a reelection turning point of 4.582 thousand real PEN per capita, well beyond the 99th percentile. The full-domain fit is therefore U-shaped. That its upward-sloping branch concerns only a few districts is substantively meaningful: exceptionally large rents may alter the value of holding office in ways that smaller transfers do not, as emphasized in models of the

[48] Several studies in Latin America document political budget cycles in intergovernmental transfers. For an overview, see Eslava (2006).

[49] In the quadratic election-year specification, the linear transfer coefficient is 0.013 (standard error 0.032) for not running, 0.050 (standard error 0.033) for running and losing, and -0.063 (standard error 0.024) for running and winning. The quadratic offset is divided between reductions in non-entry and in running and losing. The small number of high-transfer observations makes the relative contribution of these two margins difficult to estimate precisely.

political resource curse (Caselli and Cunningham, 2009). These few districts are central to the interpretation, although their number limits how precisely I can characterize the reversal.[50]

The signs of the coefficients in Table 2 are consistent with a convex response under the framework's intermediate-effectiveness case. Following Caselli (2006), the probability of remaining in power may vary non-monotonically with rents when the incumbent has an intermediate capacity to protect the position.[51] The estimates show that a linear approximation masks changing slopes over the empirically relevant range. The framework helps interpret this pattern without imposing a structural shape on the data.[52]

6.2 Political competition

Table 3 presents the political-competition results using the same fixed-effects specifications and transfer measures as Table 2. The dependent variable is one minus the Herfindahl index of candidate vote shares, scaled from 0 to 100. The index captures the dispersion of votes across candidates rather than candidate entry itself. The upper panel uses average mining transfers over the electoral cycle.

The results are consistent with the reelection evidence. Column 1 shows no statistically detectable linear relationship between average mining transfers and political competition. When I add the square of mining transfers in column 2, however, the level coefficient is -1.017 and the quadratic coefficient is 0.045; both estimates are statistically significant. The point estimates remain almost unchanged after controlling for the logarithm of real mineral production in column 3 (-1.016 and 0.045). The estimated production association is itself close to zero: -0.003 points on the 0–100 competition index per unit of the logarithmic production measure (standard error 0.056).

The right column of Figure 5 applies the same calculations to political competition using average transfers over the electoral cycle. At the sample mean of 127 PEN per capita, the estimated marginal effect is -1.005 points on the 0–100 competition scale, with a 95% confidence interval from -1.808 to -0.202. This estimate is equivalent to approximately 1.2% of average political competition, which is 83.07. The point estimate remains negative through the 99th percentile of average transfers, or 1.818 thousand PEN per capita, where it equals -0.854 and its confidence interval remains below zero.

[50] Only 10 of 4,487 observations, representing seven districts, lie above the turning point. Online Appendix Figure A.5 displays the fitted relationship and marginal-effect curve over the full support. The marginal effect becomes statistically positive only above approximately 6.44 thousand PEN per capita, a range containing seven observations from four districts.

[51] See Figure 3 in Caselli (2006). In that model, the parameter γ captures the incumbent's ability to transform rents into resources that increase the probability of remaining in power.

[52] The results differ from evidence for Brazilian municipalities. Monteiro and Ferraz (2012) find that oil royalties increase short-run mayoral reelection by approximately 32% relative to the mean, but the effect disappears in the medium term. When the elections are pooled, they find no average effect, a result similar to the linear election-year specification in column 1 of Table 2. Brollo et al. (2013) find that intergovernmental transfers increase reelection by approximately 7%. Their variation is not tied to natural-resource exploitation, however, which limits direct comparison with the present setting.

The point estimates imply a turning point of 11.405 thousand real PEN per capita. The positive quadratic coefficient therefore indicates that the negative marginal effect attenuates as transfers increase. A possible reversal at extraordinary transfer levels is substantively important: these rare districts are precisely where large resource rents may change political incentives most sharply. Because few observations lie above the turning point, the slope in that range remains less precisely estimated.[53]

Columns 4–6 repeat the analysis for the restricted samples used in Table 2. The magnitude and statistical precision of the level and quadratic coefficients change little across these samples. Across both transfer timings and these geographic samples, the production coefficients range from -0.003 to 0.013 points on the competition index per log point and are not statistically significant.

The lower panel of Table 3 reports estimates based on election-year transfers. The coefficients are imprecisely estimated in the all-district specifications and most geographic subsamples. Only the producing-province sample in column 6 produces negative and positive level and quadratic coefficients that are marginally significant. The contrast with the reelection results suggests that potential candidates respond more to the fiscal resources accumulated over the electoral cycle than to transfers received in the election year. This pattern is consistent with local politicians observing the municipality's fiscal position before deciding whether to compete, but the evidence does not isolate that mechanism.

Online Appendix Table A10 examines the number of mayoral candidates as a complementary measure of nominal entry. At ordinary transfer levels, entry increases modestly while candidate vote shares become less dispersed. This contrast illustrates why the number of candidates is not an alternative measure of effective competition: additional fringe candidates may enter without reducing the concentration of votes among the leading contenders.[54]

Taken together, the competition results show a convex association with mining transfers that a constant-slope model misses. The decline is well estimated over ordinary transfer levels, while the possible reversal concerns the few districts receiving exceptional rents. The transfer coefficients change little when I control for observed production; the non-producing-district analysis below examines the same pattern where local extraction cannot be the direct channel.[55]

[53] Only two of 4,149 observations, representing two districts, exceed the turning point. The marginal effect becomes statistically positive only above approximately 14.29 thousand PEN per capita, the same two-observation range. Online Appendix Figure A.5 displays the fitted relationship and marginal effect over the full transfer support.

[54] The mean district-election has between 7.40 and 7.43 candidates in the two all-district samples. In the linear specification, an additional 1,000 PEN per capita in either average or election-year transfers is associated with 0.20 additional candidates. The quadratic estimates imply positive marginal effects throughout the bottom 99% of the transfer distribution, and the sign remains positive among districts that never produce minerals, although the linear estimates in that sample are imprecise. Moving from the median to the 90th percentile predicts approximately 0.10 additional candidates using average transfers and 0.13 using election-year transfers.

[55] The results differ from those of Monteiro and Ferraz (2012), who find that oil royalties reduce several measures of political competition in the short run but have no detectable medium-run effect.

6.3 Identification and robustness checks

I next evaluate the principal threats to the interpretation of the political results. First, I use non-producing recipients to separate fiscal transfers from local extraction. I then examine fiscal substitution, the population denominator, spatial dependence in inference, upper-tail influence, and the adequacy of the quadratic approximation.

Table 4 presents the restricted-sample evidence for reelection; Online Appendix Table A11 reports the corresponding political-competition estimates. Column 1 reproduces the all-district benchmark. Column 2 removes every district that records positive physical mineral production in any year between 1996 and 2010. The classification is permanent, so a producer cannot reenter the sample in a year with zero recorded production. The election-year coefficients become larger in absolute magnitude: the level coefficient changes from -0.064 to -0.124, while the quadratic coefficient changes from 0.007 to 0.016. Both remain statistically significant.

Column 3 restricts the comparison to never-producing districts in producing regions, while column 4 uses the narrower set of never-producing districts in producing provinces. The election-year coefficients remain negative for the transfer level and positive for its square: -0.114 and 0.015 in producing regions, and -0.143 and 0.018 in producing provinces. Because none of these districts hosts mineral extraction during the sample period, its mayor cannot generate the observed relationship by changing production within the district. The results therefore rule out the direct local politics–production–transfer channel in the restricted samples, although they cannot eliminate broader regional spillovers.

Online Appendix Table A12 examines fiscal substitution using executed FONCOMUN, the principal universal intergovernmental transfer. Mining transfers are associated with a small contemporaneous reduction in FONCOMUN among never-producing districts, but total executed municipal budgets increase rather than decline. Moreover, controlling for FONCOMUN leaves the election-year reelection coefficients nearly unchanged. Fiscal substitution is therefore too small to explain the central non-monotonic pattern.[56]

A related concern is that annual population, which enters the per capita denominator, may itself respond to transfers or public-good provision. Online Appendix Table A13 reconstructs both political treatment measures using each district's 2001 population from INEI's official pre-boom projection series (Instituto Nacional de Estadística e Informática, 2002). The negative level and positive quadratic coefficients remain statistically significant for reelection in both the all-district and never-producer samples and for political competition in the all-district sample.[57] Using a fixed

[56] In the linear election-year specification, the FONCOMUN coefficient is -0.008 (standard error 0.002): an additional 1,000 real PEN per capita in mining transfers is associated with approximately 8 PEN less FONCOMUN, an offset below one percent. For average transfers over the electoral cycle, the corresponding coefficient is -0.001 (standard error 0.003) and is not statistically distinguishable from zero. Adding FONCOMUN as a control yields election-year reelection coefficients of -0.128 for transfers and 0.017 for transfers squared among never-producing districts.

[57] For reelection, the fixed-denominator coefficients are -0.077 and 0.010 in the all-district sample and -0.115 and 0.015 among never-producing districts. The corresponding political-competition coefficients are -1.055 and 0.054. The year 2001 predates the post-2003 mineral-price boom and covers the complete preferred estimation samples under

denominator removes post-boom population changes from the treatment measure, but it does not measure migration directly or rule out migration-related effects through other channels.

Online Appendix Table A14 examines alternative inference while leaving the point estimates unchanged. Clustering at the province level preserves the all-district political-competition result and the especially strong never-producer reelection result, while confirming the weaker precision of the restricted political-competition specification.[58]

Online Appendix Table A15 provides a complementary upper-tail influence check. Excluding the largest producing districts leaves the sign and statistical precision of both reelection coefficients essentially unchanged. For political competition, excluding the top one percent preserves both coefficients and their precision, while the more aggressive five-percent exclusion preserves the signs but reduces the precision of the quadratic term. The reelection result is therefore not driven by the largest producing districts, although the political-competition result is more sensitive to the most restrictive exclusion.[59]

I finally examine the adequacy of the quadratic approximation. Table 5 compares it with cubic and quartic specifications for reelection; Online Appendix Table A16 reports the corresponding political-competition results. Nested-model tests do not reject the quadratic in favor of either higher-order polynomial. Online Appendix Figures A.6 and A.7 provide a complementary assessment using the Baltagi–Li semiparametric fixed-effects estimator. The fitted relationships preserve the broad decline at ordinary transfer levels. At exceptional levels, some curves turn upward, but their shape varies across polynomial degrees, reflecting the small number of highly exposed districts.[60] Taken together, these exercises support the quadratic as a parsimonious summary of changing slopes, while the precise shape at exceptionally high rents remains sensitive to the fit.

6.4 Instrumental variables with an imperfect instrument and sensitivity bounds

I next use Mining Canon and its square as imperfect instruments for total mining transfers and their square. Canon is distributed according to statutory allocation rules and is less directly tied than royalties to contemporaneous local production. It nevertheless remains a component of the aggregate treatment and may be correlated with unobserved determinants of political outcomes. Rather than impose an exact exclusion restriction, I apply the approach proposed by Nevo and Rosen (2012) and examine how the estimates change when the instrument is allowed to correlate

the harmonized geography.

[58] For reelection, the province-clustered p-values for the transfer level and its square are 0.008 and below 0.001 in the all-district sample, and 0.004 and below 0.001 among never-producing districts. For political competition, the corresponding all-district p-values are both 0.001. Among never-producing districts, the level term remains statistically significant under province clustering, while the quadratic term remains imprecise.

[59] Among the 159 districts with positive mineral production, the exercise excludes either the two districts in the top one percent or the eight districts in the top five percent of average real production.

[60] For reelection, the p-values for the cubic and quartic comparisons are 0.594 and 0.353. The corresponding p-values for political competition are 0.887 and 0.795. I estimate the semiparametric model for the 2002–2010 panel using local polynomial degrees one through four and an Epanechnikov kernel. Standard errors for the parametric component are clustered at the district level. The exercise assesses the shape of the relationship rather than replacing the quadratic baseline.

with the structural error.

Table 6 reports the sensitivity analysis. Column 1 reproduces the panel fixed-effects estimate, while column 2 presents the Mining Canon IV specification. The IV coefficients are −0.073 for the transfer level and 0.008 for its square, compared with −0.064 and 0.007 in the fixed-effects model. Both IV coefficients are statistically significant at the one-percent level.[61]

Columns 3–7 present the adjusted-instrument estimates for $\lambda \in \{0.1, 0.3, 0.5, 0.7, 0.9\}$, where larger values allow greater departures from the exclusion restriction. The negative level and positive quadratic coefficients remain statistically detectable through $\lambda = 0.7$, although first-stage relevance declines sharply at larger values of λ. At $\lambda = 0.9$, the adjusted instruments are weak for both endogenous terms and the estimates become uninformative. The convex pattern therefore survives moderate departures from instrument validity, while the most demanding sensitivity values substantially weaken the first stage.

Taken together, the imperfect-instrument exercise preserves the sign and approximate magnitude of the fixed-effects estimates over the range in which the adjusted instruments remain informative. I therefore place primary weight on the panel fixed-effects results and, especially, on the estimates for never-producing recipient districts.

Online Appendix Table A21 reports the corresponding sensitivity estimates for political competition. The Mining Canon IV coefficients are −1.020 for average transfers and 0.047 for their square, and both are statistically significant at the five-percent level or better. The signs and precision persist through $\lambda = 0.7$, while the estimates at $\lambda = 0.9$ are imprecise. Table 6 also reports the reelection first-stage F statistics alongside the second-stage estimates so that the loss of relevance across the sensitivity sequence can be assessed directly.

6.5 Provision of public goods

I next examine whether the fiscal windfall changed the provision of local public goods. Table 7 reports panel fixed-effects estimates for eight outcomes, using the available RENAMU coverage for each outcome through 2010. Every specification includes the level and square of mining transfers, the production control, and district and year fixed effects.

The estimates reveal an inverted-U relationship for public lighting, garbage collection, access to municipal security services, security personnel, and security stations. For example, the coefficient on mining transfers is 0.055 for municipal security access, while the coefficient on its square is −0.001; both estimates are statistically significant at the one-percent level. The corresponding estimates for security personnel are 0.220 and −0.005. By contrast, I find no comparable relationship for access to potable water or libraries.[62] The production control is statistically distinguishable from

[61] The equation-specific first-stage F statistics are 235.51 for transfer levels and 29.03 for squared transfers. These statistics document strong joint relevance in each first-stage equation, although they are not conditional weak-identification diagnostics for a model with two endogenous regressors. Online Appendix Tables A17 and A18 report the complete first-stage coefficients and joint F statistics for reelection; Tables A19 and A20 provide the corresponding diagnostics for political competition.

[62] Qualitative evidence identifies potable water as a priority for the population (Arellano Yanguas, 2011). One

zero only for security personnel and libraries.

These results differ from evidence finding few changes in public-good provision after resource windfalls. Caselli and Michaels (2013) study the effect of oil royalties on housing, urban services, infrastructure, and educational and health inputs, while Monteiro and Ferraz (2012) analyze a related Brazilian setting. Direct comparisons are difficult because the allocation of responsibilities across levels of government differs across countries.[63] Nevertheless, the Peruvian evidence indicates that some municipal services respond to the transfer boom, although the estimated changes remain modest relative to the fiscal windfall.

Online Appendix Table A22 reports Mining Canon IV estimates for public-good provision, while Tables A23 and A24 extend the analysis to local infrastructure and roads. The fixed-effects estimates show an inverted-U pattern for basic health facilities, stadiums, and soccer fields, but not for most measures of road construction. Some road-cost outcomes respond to transfers, although the evidence is not uniform across categories.[64] The Mining Canon IV estimates in Tables A22–A24 are generally less precise, and I treat them as sensitivity exercises rather than as the primary evidence.

6.6 Clientelism

An alternative response available to local politicians is municipal employment, a channel emphasized by Robinson et al. (2006) and Robinson and Verdier (2013). Table 8 examines five occupational groups: officials, professionals, technicians, security workers, and janitors. For each group, the first specification controls for production, while the second excludes every district with positive mineral production between 1996 and 2010.

The fixed-effects estimates produce positive level coefficients for all five groups and negative quadratic coefficients for four of them in the full sample. The exception is technicians, for whom the quadratic estimate is imprecise. For officials, an additional 1,000 PEN per capita in transfers is associated with an initial increase of 0.246 employees per 1,000 inhabitants, relative to a sample mean of 0.60. The negative quadratic coefficient implies that this response declines as transfers increase. The pattern is especially clear among never-producing districts, where the level coefficients are positive and the quadratic coefficients are negative and statistically significant for all

potential explanation for the absence of a detectable relationship is the technical complexity of water projects and the requirement that public investments be technically and financially viable. Smaller municipalities often cannot meet these requirements, and the projects' long implementation horizons may offer limited electoral returns to an incumbent mayor. The library coefficient may reflect the "Mining Program of Solidarity with the People," implemented during Alan Garcia's second administration (2006–2011). Under this program, mining companies financed public goods in communities near their operations; for example, the Yanacocha mine helped establish libraries in 184 educational institutions in Cajamarca between 2007 and 2008.

[63] In Peru, several public goods are jointly provided by different levels of government, with the central government retaining an especially important role in education and health.

[64] I find no relationship between mining transfers and the availability of hospitals, health centers, or polyclinics. The relationship with basic health facilities is non-monotonic but modest. The evidence is clearer for sports infrastructure, including stadiums and soccer fields. For roads, I find no systematic relationship with the physical quantities of repaired roads, sidewalks, or rural roads, although some cost measures display a non-monotonic pattern.

five occupational groups. The employment response therefore cannot be attributed solely to jobs generated directly by mineral production.

This evidence is consistent with, but does not by itself identify, the use of municipal employment as a political instrument.[65] Online Appendix Table A25 shows that the nonlinear pattern is strongest for contracted and temporary employees, particularly among never-producing districts; the response for appointed employees is weaker.[66] The Mining Canon IV estimates in Table A26 preserve the positive-level and negative-quadratic pattern for several occupational groups, although their precision varies. Taken together, the results are consistent with an expansion of municipal employment at moderate transfer levels that weakens near the upper end of the transfer distribution.

6.7 Local government expenditures

I next examine how municipalities allocate the additional fiscal resources. Table 9 reports panel fixed-effects estimates for the nine principal functional expenditure categories. All outcomes are executed expenditures per capita in real 2001 Lima PEN and follow the common Ministry of Economy and Finance classification observed through 2010.

The level coefficient is positive and statistically significant for every function. The quadratic coefficient is negative for all nine categories and statistically significant for eight; health and sanitation is the exception. The largest initial responses occur in transport and planning. At the bottom of the transfer distribution, an additional 1,000 PEN per capita in transfers is associated with approximately 253 PEN per capita in transport expenditure and 251 PEN in planning expenditure. Agriculture follows, with a coefficient of 167 PEN. The negative quadratic terms imply that these increases become progressively smaller as transfers rise.

The prominence of infrastructure-related functions is consistent with the legal framework governing Canon resources. Transport projects are visible, intensive in local labor, and may therefore have electoral value.[67] The agricultural response may similarly reflect attempts to compensate rural residents for the costs associated with mining, often through irrigation projects or the distribution of agricultural inputs (Arellano Yanguas, 2011).[68]

[65] The legal framework restricts the use of Canon transfers and mining royalties to hire workers. Nevertheless, qualitative evidence indicates that municipalities charged some personnel expenditures to investment projects under categories such as "institutional strengthening" (Arellano Yanguas, 2011). This accounting practice allowed municipalities to expand temporary employment without formally violating the restrictions on current expenditure.

[66] This distinction is consistent with temporary contracts being more easily adjusted for political purposes. It also differs from the Brazilian evidence in Monteiro and Ferraz (2012), who estimate an increase of seven municipal employees per 1,000 inhabitants after an oil windfall but find no change in the education or contract composition of public employment. Because Mining Canon is an imperfect instrument for total mining transfers, I interpret the corresponding estimates in Table A26 as sensitivity evidence.

[67] A salient example is the "Pilot Plan for Maintenance of Public Infrastructure" in San Marcos, Ancash. According to Salas (2010), the municipality employed almost every working-age resident and paid a wage nearly four times the prevailing agricultural wage in exchange for maintaining roads and other basic infrastructure. Salas argues that the program also helped the mayor avoid a recall election.

[68] Online Appendix Table A27 reports results by economic classification. Goods and services, investment, and other capital expenditure display the clearest nonlinear responses. Table A28 reports the Mining Canon IV estimates

6.8 Living standards

I finally examine whether the fiscal boom affected household living standards. Table 10 reports fixed-effects estimates for monthly household income and consumption per capita using ENAHO data through 2010. Both outcomes are expressed in real 2001 Lima prices. Income responds more quickly to short-run economic conditions, whereas consumption is commonly viewed as a better measure of longer-run well-being (Deaton, 1997).

The linear specification indicates that an additional 1,000 PEN per capita in mining transfers is associated with an increase of 43.9 PEN in monthly income per capita, relative to a sample mean of 479 PEN. The estimate is statistically significant at the one-percent level. In the quadratic specification with the production control, the level coefficient is 115.7 and the quadratic coefficient is -2.46. The former is statistically significant at the one-percent level, while the latter is significant at the five-percent level. The income estimates therefore suggest diminishing gains as transfers increase.

The production coefficient is not statistically distinguishable from zero, and the income estimates remain similar after excluding Lima or restricting the sample to producing regions and provinces. The consumption coefficients are much smaller. In the all-district quadratic specification, the level term is not statistically distinguishable from zero and the quadratic term is significant only at the ten-percent level. Both terms become statistically detectable in the producing-region and producing-province samples, although the transfer level is significant only at conventional marginal levels. The accumulated evidence therefore points to a clearer short-run income response and a weaker consumption response.[69][70]

7 Discussion of Results

A growing empirical literature examines the political and economic consequences of resource booms. Studies analyze oil (Caselli and Michaels, 2013; Monteiro and Ferraz, 2012; Dube and Vargas, 2013; Vicente, 2010), mining (Maldonado, 2011; Aragon and Rud, 2013; Arellano-Yanguas, 2011; Loayza and Rigolini, 2016; Pebe et al., 2017; Zambrano et al., 2014), and coca cultivation (Angrist and Kugler, 2008; Dube and Vargas, 2013).

by function. The results are less uniform, particularly for the quadratic terms, and the imperfect nature of the instrument limits their direct causal interpretation.

[69] Online Appendix Figures A.8 and A.9 provide complementary descriptive evidence: average income and consumption increased more rapidly in producing districts, although the consumption pattern is less pronounced. Figures A.10 and A.11 report larger declines in total and extreme poverty among producing districts. These figures present unweighted group means and therefore should not be interpreted as causal estimates.

[70] For Brazil, Caselli and Michaels (2013) find no effect of the oil boom on per capita household income, with only weak evidence at the bottom of the income distribution. For Peru, Loayza and Rigolini (2016) find higher consumption and lower poverty in producing districts, together with higher inequality and no comparable benefits in neighboring non-producing districts. Zambrano et al. (2014) report faster reductions in poverty and inequality in mining-abundant districts and heterogeneous responses by initial conditions and transfer exposure. Aragon and Rud (2013) document income gains near the Yanacocha mine. These studies focus primarily on mining activity or abundance and household outcomes; the fiscal-transfer relationship examined here is therefore complementary.

Related work examines fiscal windfalls unrelated to extraction (Brollo et al., 2013). By contrast, the consequences of a resource boom may also depend on the characteristics of the resource and the technology used to extract it. In line with Dube and Vargas (2013), labor-intensive and capital-intensive activities may affect local economies through different channels. Large-scale mining is generally capital intensive, so its effect on household well-being may operate primarily through fiscal rents rather than local labor demand. Artisanal gold production in parts of the Peruvian Amazon provides an important exception because it is labor intensive and may affect household income directly through market mechanisms. The unusually large decline in poverty in that region is consistent with this possibility.[71]

Coca cultivation provides a related example of a labor-intensive resource activity. Angrist and Kugler (2008) show that the expansion of coca production in Colombia, following aerial interdiction in Peru and Bolivia, increased self-employment income, rural employment, and the labor supply of young men.[72]

Finally, the estimated improvements in public-good provision and living standards are modest relative to the magnitude of the boom. This discrepancy raises questions about how municipalities use mining transfers. The evidence points to an expansion of municipal employment and local infrastructure intensive in unskilled labor. These expenditures may have limited productive returns but considerable political value.[73] Corruption by political authorities and municipal officials, as emphasized by Maldonado (2011), provides another potential explanation.[74] The combination of distorted political incentives and weak local administrative capacity helps explain why the estimated effects remain modest relative to the increase in mining transfers.

[71] Madre de Dios provides the clearest example. Between 2001 and 2010, its poverty rate fell from 36.7% to 8.7%, compared with a national decline from 54.8% to 31.3%. The region is characterized by extensive informal gold production. For a detailed discussion, see INEI (2011).

[72] The evidence in Brollo et al. (2013) is more difficult to interpret from this perspective. Their fiscal shock is not associated with the exploitation of a natural resource. The economic properties of a resource—including the intensity with which it uses labor or capital—are closely related to its political properties and the scope for politicians to appropriate rents. For this reason, an increase in intergovernmental transfers need not capture the complete set of mechanisms associated with a "resource curse."

[73] The Peruvian media has repeatedly documented "white elephants" and highly visible construction in mineral-rich municipalities. In Yarabamba, Arequipa, for example, three stadiums were built for a population of approximately 1,200 residents; the largest could seat 3,000 people. Other municipalities used transfers to renovate central squares or construct monuments to a soccer referee in Tumbes, maca in Junin, a hat in Cajamarca, and a lizard in Tumbes. Beyond their eccentricity, projects of this type redistribute resources through public employment in infrastructure intensive in unskilled labor and may therefore generate high electoral returns.

[74] Local media reports describe numerous allegations of corruption against district and provincial mayors in mineral-rich regions. The Comptroller General of the Republic investigated the mayors of San Marcos and Chavin in Ancash for the alleged misuse of public funds, while the Ministry of Economy and Finance restricted access to public funds in several municipalities in Ancash, Cajamarca, Tumbes, Pasco, and Puno. According to the Comptroller, more than 3,000 public officials had been accused of corruption since 2009.

8 Concluding Remarks

This paper analyzes how a mineral-resource boom is associated with the incentives of local politicians, public-good provision, municipal employment, and citizens' well-being. Recent research has advanced our understanding of the political and economic consequences of resource booms, while leaving open how local political incentives shape the use of large fiscal windfalls. One recurring finding is that these windfalls generate only small average changes in public-good provision and household well-being. Given the magnitude of the revenues, this pattern motivates examining whether the relationship varies with the size of the rents.

I find lower reelection and less dispersed vote shares at low and average levels of mining transfers, with marginal effects that become less negative as transfers increase. The fitted slopes turn positive only among a handful of districts with extraordinary transfers, where political incentives may differ most sharply from those in the typical municipality. The pattern remains similar when I use alternative production measures, restrict the analysis to non-producing recipients, examine alternative functional forms, and allow for dependence among districts within provinces.

These patterns are consistent with local politicians responding strategically to the resource boom, although the reduced-form evidence does not isolate a unique political mechanism. I also find increases in several dimensions of public-good provision, municipal employment, and short-run household income at moderate transfer levels. These changes are nevertheless modest relative to the fiscal resources distributed to local governments as Mining Canon and mining royalties. In line with the conceptual framework, the relationships between transfers, several public goods, and household income display changing slopes.

These results show why functional form matters for the study of resource abundance. A constant-slope approximation may conceal economically meaningful changes in the marginal relationship and, for some outcomes, produce no statistically detectable average relationship. One contribution of the paper is to document this variation across the observed transfer distribution, including the exceptional districts in which the political stakes of resource rents may be greatest.

Taken together, the results place the political allocation of rents at the center of the local resource-curse debate. At moderate transfer levels, mining revenues are associated with gains in several public services and household income, but those gains are modest relative to the fiscal windfall, while electoral responses change with the size of the rents. The development value of resource wealth therefore depends on how local governments use it, not simply on how much revenue they receive.

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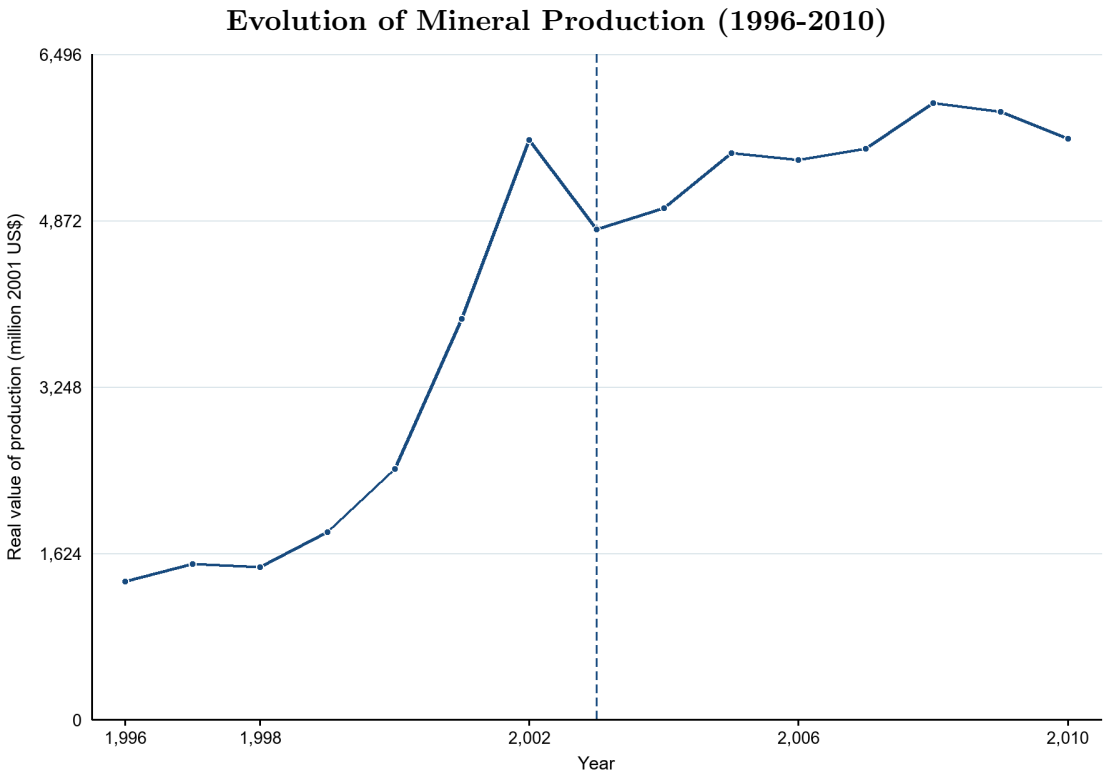


Figure 1. Author's calculations based on data from the Ministry of Energy and Mines. The series reports the aggregate value of district-level mineral production in millions of constant 2001 US dollars. The vertical line marks the onset of the international mineral-price boom in 2003.

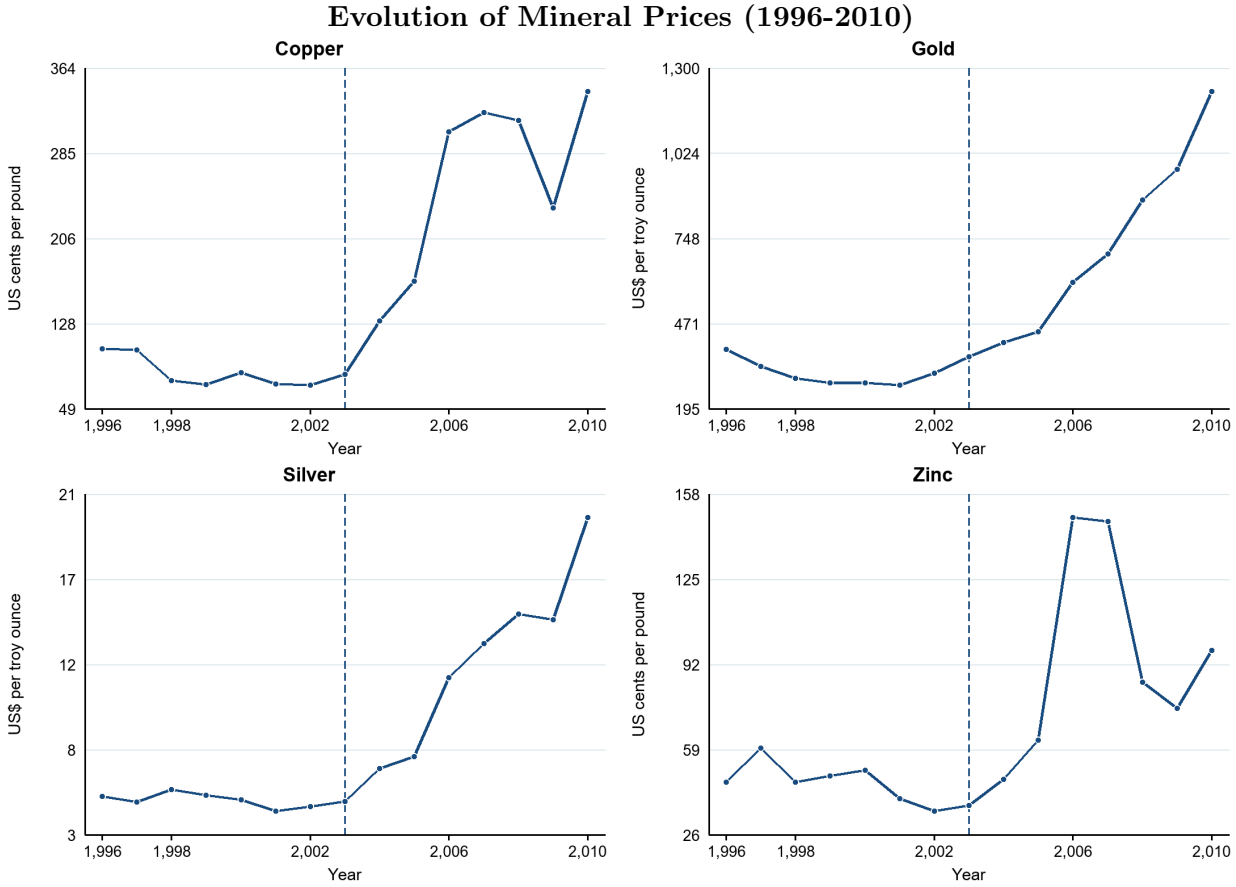


Figure 2. Author's calculations based on the international benchmark-price series used to value mineral production. The panels report prices for copper, gold, silver, and zinc from 1996 through 2010. Copper and zinc are measured in US cents per pound; gold and silver are measured in US dollars per troy ounce. The vertical line marks the onset of the price boom in 2003.

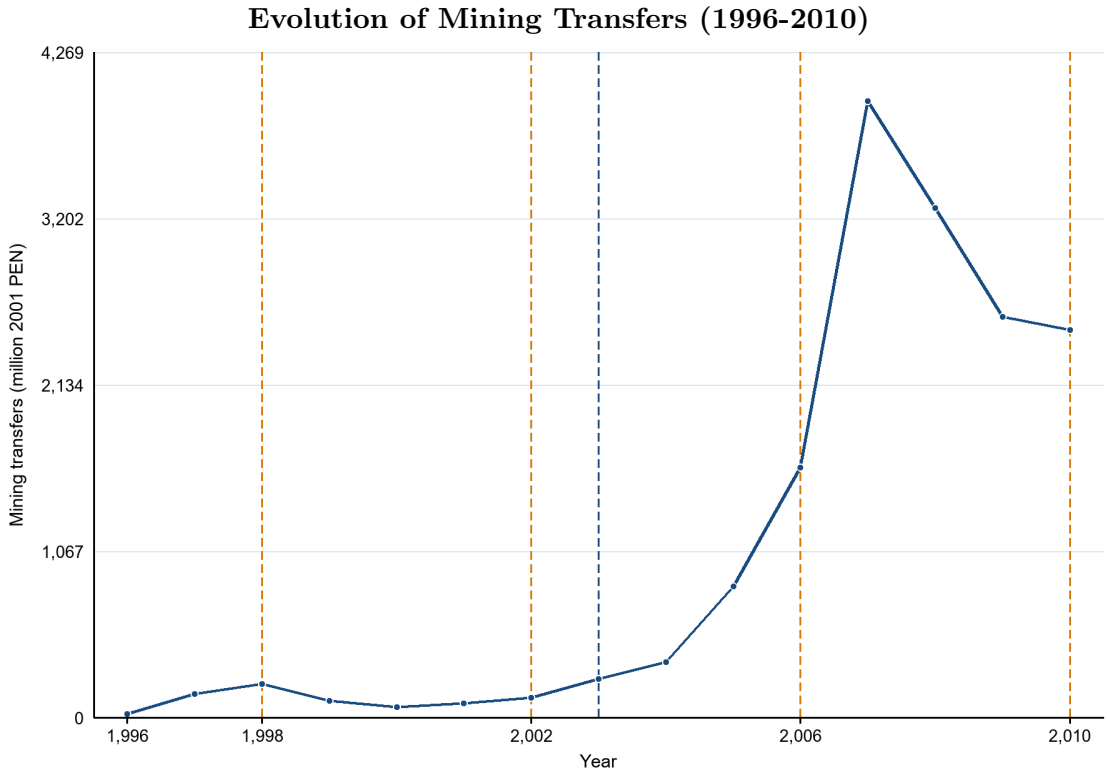


Figure 3. Author's calculations based on data from the Ministry of Economy and Finance. The series is the annual sum of Mining Canon and mining royalties received by district governments, measured in millions of PEN at 2001 Lima prices. The blue line marks the onset of the price boom in 2003; the orange lines mark municipal election years.

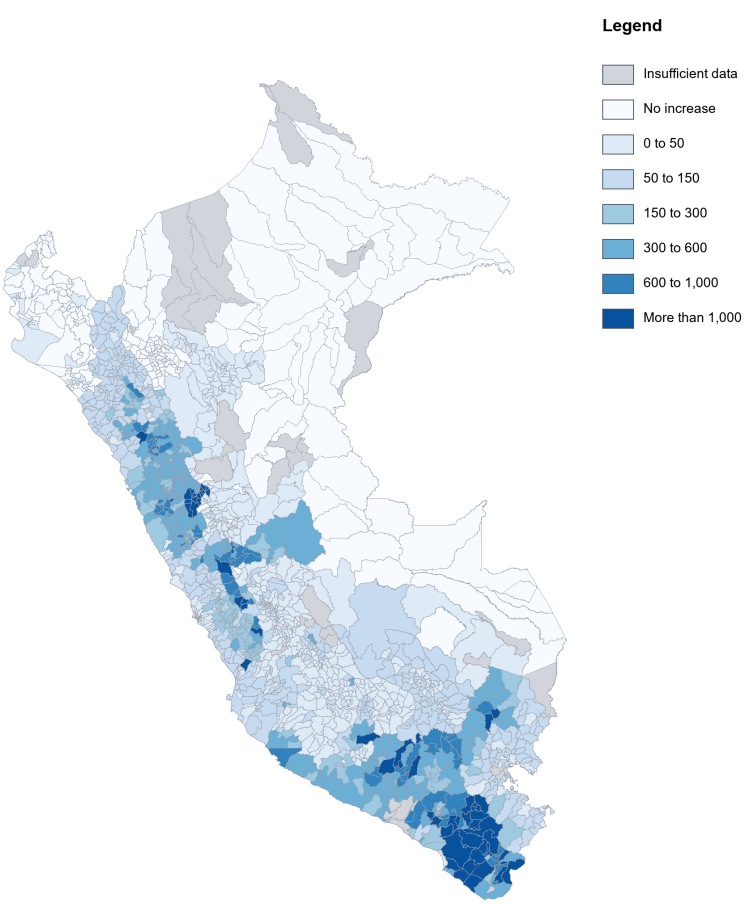


Figure 4. Author's calculations based on data from the Ministry of Economy and Finance. The map reports the difference between average real mining transfers per capita in 2008–2010 and 1996–1998. Values are measured in 2001 PEN per capita. District averages require at least two nonmissing years in each three-year window, and districts without sufficient coverage are shaded gray. Geographic identifiers harmonize district boundaries over the study period. The map is descriptive and does not represent an estimated causal effect.

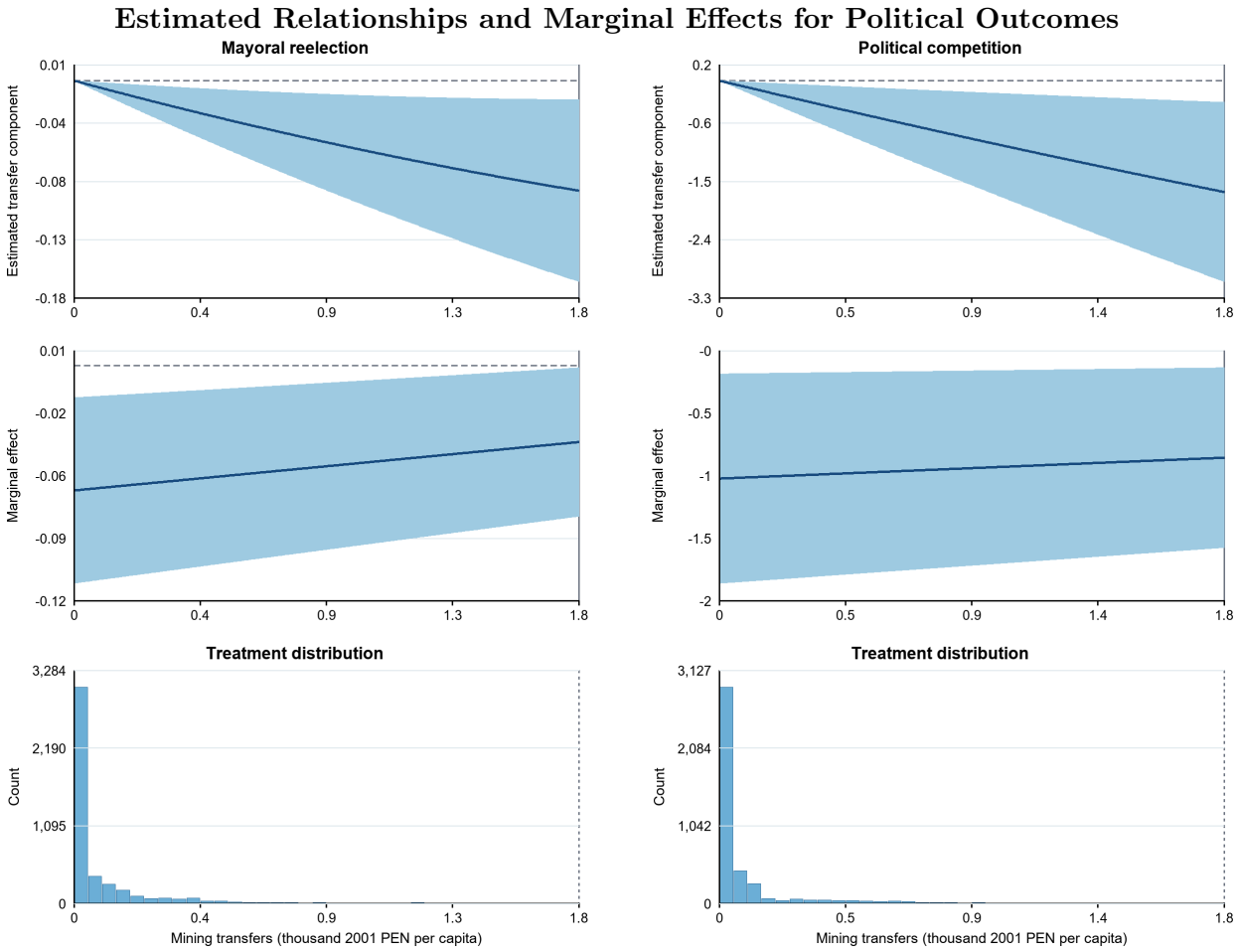


Figure 5. Author's elaboration. The left column uses election-year mining transfers and mayoral reelection; the right column uses average transfers over the electoral cycle and political competition. The upper panels plot the estimated transfer component $\widehat{\beta}_1 R + \widehat{\beta}_2 R^2$, relative to zero transfers. The middle panels plot the marginal effect $\widehat{\beta}_1 + 2\widehat{\beta}_2 R$. Shaded areas are pointwise 95% confidence intervals computed from the joint district-clustered covariance matrix, and the lower panels show the treatment distributions. Gray vertical lines mark the 99th percentiles. Each horizontal axis ends at that percentile, while all models are estimated on the full estimation sample. Full-support curves are reported in Appendix Figure A.5. Mining transfers are measured in thousands of 2001 PEN per capita.

Table 1. Contextual Comparison by District-Year Exposure Group

	Non-producing recipients	Producing district-years	Non-recipient district-years
Panel A: Political outcomes			
Mayor reelected	0.19	0.18	0.11
Political competition index (0–1)	0.82	0.84	0.81
Panel B: Transfers and municipal budget			
Mining transfers per capita, mean	105.34	553.97	0.00
Mining transfers per capita, p10	0.09	0.73	0.00
Mining transfers per capita, p25	0.85	4.84	0.00
Mining transfers per capita, p50	7.72	49.08	0.00
Mining transfers per capita, p75	56.37	380.31	0.00
Mining transfers per capita, p90	214.41	1,055.36	0.00
Mining transfers per capita, p99	1,434.70	11,246.53	0.00
Executed municipal budget per capita	531.17	1,470.42	318.04
Panel C: Mineral production			
Real value of mineral production	0.00	62,064,651.34	0.00
Copper	0.00	20,677,088.80	0.00
Zinc	0.00	14,286,012.35	0.00
Lead	0.00	1,903,010.76	0.00
Tin	0.00	1,994,002.93	0.00
Molybdenum	0.00	563,471.11	0.00
Silver	0.00	5,841,213.12	0.00
Gold	0.00	15,899,801.22	0.00
Iron	0.00	900,051.05	0.00
Panel D: Predetermined district characteristics, 1993 census			
Population	12,481.56	9,968.65	16,724.56
Rural population (%)	57.62	54.18	56.24
Children ages 0–15 (%)	40.55	40.72	43.67
Child malnutrition rate	55.62	52.45	55.95
Population without sewerage or latrine (%)	41.91	40.49	47.27
Population without piped water (%)	50.95	47.54	60.70
Population without electricity (%)	73.92	61.66	70.14
Female illiteracy rate	33.54	29.72	28.16
Altitude (meters)	2,387.89	2,857.60	993.31

Notes: The mutually exclusive exposure groups use district-year information from 1998–2010. A producing observation records positive local mineral production; a non-producing recipient receives positive mining transfers without local production; a non-recipient has neither. Political outcomes are observed in ordinary election years. Monetary amounts are real 2001 Lima PEN per capita, while mineral production is valued at 2001 prices. Panel D uses predetermined 1993 census characteristics. Sources: Ministry of Economy and Finance, Ministry of Energy and Mines, ONPE/JNE, and the 1993 Population Census.

Table 2. Impact of Natural Resource Booms on Reelection

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average transfers over the electoral cycle						
Average mining transfers per capita	-0.026^{***}	-0.035	-0.034	-0.030	-0.031	-0.020
	(0.010)	(0.026)	(0.026)	(0.025)	(0.026)	(0.027)
Average mining transfers per capita squared		0.001	0.001	0.000	0.000	-0.000
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(1 + real mineral production)			-0.002	-0.002	-0.002	-0.002
			(0.004)	(0.005)	(0.004)	(0.004)
Mean of dependent variable	0.183	0.183	0.183	0.169	0.180	0.197
Observations	4,092	4,092	4,092	3,676	3,318	1,912
Districts	1,575	1,575	1,575	1,413	1,285	739
Within R-squared	0.015	0.015	0.015	0.016	0.012	0.010
Panel B: Transfers in the election year						
Mining transfers per capita	0.007	-0.065^{***}	-0.064^{***}	-0.060^{**}	-0.059^{**}	-0.067^{***}
	(0.018)	(0.024)	(0.024)	(0.024)	(0.025)	(0.026)
Mining transfers per capita squared		0.007^{***}	0.007^{***}	0.007^{***}	0.007^{***}	0.007^{***}
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log(1 + real mineral production)			-0.001	-0.001	-0.001	-0.002
			(0.004)	(0.004)	(0.004)	(0.004)
Mean of dependent variable	0.182	0.182	0.182	0.170	0.179	0.192
Observations	4,487	4,487	4,487	4,032	3,648	2,106
Districts	1,588	1,588	1,588	1,424	1,297	747
Within R-squared	0.014	0.017	0.017	0.019	0.015	0.016
Production control	No	No	Yes	Yes	Yes	Yes
Excludes Lima and Callao	No	No	No	Yes	No	No
Producing regions only	No	No	No	No	Yes	No
Producing provinces only	No	No	No	No	No	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Election fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is an indicator for the unconditional reelection of the incumbent mayor. Huber–White standard errors clustered by district are reported in parentheses. Mining transfers are thousands of real 2001 Lima PEN per capita. Producing-area columns use the 1996–2010 producer classification constructed from the validated fixed-1998 identifiers. The complete-case panels are unbalanced and cover the 2002–2010 elections. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. Impact of Natural Resource Booms on Political Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average transfers over the electoral cycle						
Average mining transfers per capita	−0.378	−1.017**	−1.016**	−1.012**	−0.946**	−1.135**
	(0.234)	(0.414)	(0.414)	(0.417)	(0.418)	(0.458)
Average mining transfers per capita squared		0.045***	0.045***	0.045***	0.042**	0.051***
		(0.017)	(0.017)	(0.017)	(0.017)	(0.018)
Log(1 + real mineral production)			−0.003	0.011	−0.002	0.001
			(0.056)	(0.059)	(0.056)	(0.056)
Mean of dependent variable	83.070	83.070	83.070	83.207	83.699	83.500
Observations	4,149	4,149	4,149	3,730	3,361	1,937
Districts	1,595	1,595	1,595	1,432	1,300	748
Within R-squared	0.131	0.133	0.133	0.140	0.143	0.152
Panel B: Transfers in the election year						
Mining transfers per capita	−0.297	−0.355	−0.358	−0.335	−0.298	−0.502
	(0.193)	(0.392)	(0.395)	(0.397)	(0.395)	(0.426)
Mining transfers per capita squared		0.006	0.006	0.005	0.002	0.015
		(0.022)	(0.022)	(0.022)	(0.022)	(0.024)
Log(1 + real mineral production)			0.003	0.013	0.003	0.007
			(0.050)	(0.053)	(0.050)	(0.050)
Mean of dependent variable	83.047	83.047	83.047	83.164	83.665	83.546
Observations	4,545	4,545	4,545	4,087	3,692	2,132
Districts	1,608	1,608	1,608	1,443	1,312	756
Within R-squared	0.132	0.132	0.132	0.139	0.140	0.148
Production control	No	No	Yes	Yes	Yes	Yes
Excludes Lima and Callao	No	No	No	Yes	No	No
Producing regions only	No	No	No	No	Yes	No
Producing provinces only	No	No	No	No	No	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Election fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is one minus the vote-share Herfindahl index, measured from zero to 100. Huber–White standard errors clustered by district are reported in parentheses. Mining transfers are thousands of real 2001 Lima PEN per capita. Producing-area columns use the 1996–2010 producer classification constructed from the validated fixed-1998 identifiers. The complete-case panels are unbalanced and cover the 2002–2010 elections. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Mining Transfers and Reelection: Non-Producing District Samples

	(1)	(2)	(3)	(4)
Panel A: Average transfers over the electoral cycle				
Average mining transfers per capita	−0.034	−0.082***	−0.079**	−0.067*
	(0.026)	(0.032)	(0.033)	(0.035)
Average mining transfers per capita squared	0.001	0.003	0.003	0.002
	(0.001)	(0.002)	(0.002)	(0.002)
Log(1 + real mineral production)	−0.002			
	(0.004)			
Mean of dependent variable	0.183	0.180	0.176	0.194
Observations	4,092	3,745	2,971	1,565
Districts	1,575	1,438	1,148	602
Within R-squared	0.015	0.016	0.013	0.011
Panel B: Transfers in the election year				
Mining transfers per capita	−0.064***	−0.124***	−0.114**	−0.143***
	(0.024)	(0.048)	(0.049)	(0.055)
Mining transfers per capita squared	0.007***	0.016***	0.015***	0.018***
	(0.002)	(0.005)	(0.005)	(0.005)
Log(1 + real mineral production)	−0.001			
	(0.004)			
Mean of dependent variable	0.182	0.180	0.176	0.189
Observations	4,487	4,094	3,255	1,713
Districts	1,588	1,449	1,158	608
Within R-squared	0.017	0.016	0.014	0.015
Production control	Yes	No	No	No
Never-producing districts only	No	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes
Election fixed effects	Yes	Yes	Yes	Yes

Notes: The dependent variable is the unconditional reelection indicator. Column 1 reports the all-district benchmark. Columns 2–4 exclude districts with positive physical mineral production during 1996–2010; columns 3 and 4 retain those in producing regions and provinces. Standard errors clustered by district are in parentheses. Mining transfers are thousands of real 2001 Lima PEN per capita. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. Parametric Functional-Form Robustness for Reelection

	Quadratic (1)	Cubic (2)	Quartic (3)
Mining transfers per capita	−0.064***	−0.080*	−0.014
	(0.024)	(0.047)	(0.069)
Mining transfers per capita squared	0.007***	0.011	−0.027
	(0.002)	(0.009)	(0.029)
Mining transfers per capita cubed		−0.000	0.005
		(0.000)	(0.003)
Mining transfers per capita to the fourth			−0.000
			(0.000)
Production control	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
Election fixed effects	Yes	Yes	Yes
Observations	4,487	4,487	4,487
Within R-squared	0.017	0.017	0.018
p-value: added terms jointly zero	—	0.594	0.353

Notes: The dependent variable is mayoral reelection and the treatment is mining transfers in the election year. All specifications include the production control and district and election fixed effects. Standard errors are clustered by district. The sample covers 2002–2010.

Table 6. Imperfect-Instrument Estimates and Sensitivity Bounds for Reelection

	Fixed effects	Mining Canon IV	$\lambda = .1$	$\lambda = .3$	$\lambda = .5$	$\lambda = .7$	$\lambda = .9$
Mining transfers per capita	-0.064^{***}	-0.073^{***}	-0.074^{***}	-0.077^{***}	-0.084^{**}	-0.102^{**}	-0.598
	(0.024)	(0.027)	(0.028)	(0.029)	(0.033)	(0.043)	(3.428)
Mining transfers per capita squared	0.007^{***}	0.008^{***}	0.008^{***}	0.009^{***}	0.010^{***}	0.012^{***}	0.094
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)	(0.524)
Production control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,487	4,469	4,469	4,469	4,469	4,469	4,469
First-stage F: transfer level	—	235.51	202.28	142.45	83.15	22.43	2.06
First-stage F: transfer square	—	29.03	22.17	13.16	8.64	3.60	1.51

Notes: The dependent variable is mayoral reelection. Column 2 instruments total mining transfers and their square with Mining Canon and its square. Columns 3–7 report the sensitivity sequence for the indicated values of λ. All specifications include the production control and district and election fixed effects; standard errors are clustered by district.

Table 7. Impact of Natural Resource Booms on Public Goods Provision

	Water network	Public lighting	Garbage: capital	Garbage: rest	Security service	Security personnel	Security stations	Public library
Mining transfers per capita	0.007	0.023**	0.051**	0.075**	0.055***	0.220***	0.116***	-0.015
	(0.007)	(0.009)	(0.025)	(0.033)	(0.012)	(0.071)	(0.040)	(0.010)
Mining transfers per capita squared	0.000	-0.001**	-0.002***	-0.002*	-0.001***	-0.005**	-0.002**	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.000)
Log(1 + real mineral production)	-0.001	0.001	-0.001	0.004	-0.001	-0.008**	0.000	0.004**
	(0.002)	(0.001)	(0.004)	(0.006)	(0.002)	(0.003)	(0.001)	(0.002)
Mean of dependent variable	0.87	0.93	1.27	0.86	0.17	0.24	0.08	0.44
Observations	11,588	14,954	9,459	9,223	14,999	13,557	10,456	15,119
Districts	1,792	1,792	1,792	1,792	1,792	1,792	1,792	1,792
Within R-squared	0.373	0.225	0.019	0.134	0.138	0.031	0.022	0.012

Notes: Each column reports a separate district and year fixed-effects regression for the outcome named above. Standard errors are clustered by district. Mining transfers are measured in thousands of real 2001 Lima PEN per capita. Security personnel and stations are measured per 1,000 inhabitants; garbage outcomes are coverage indices; the remaining access outcomes use their harmonized RENAMU scales. Samples are outcome-specific and cover the available RENAMU years through 2010. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Impact of Resource Booms on Municipal Employment by Occupational Group

	Officials	Never producer	Professionals	Never producer	Technicians	Never producer	Security workers	Never producer	Janitors	Never producer
Mining transfers per capita	0.246***	0.335**	0.709***	0.572***	0.442***	0.382***	0.185***	0.203***	0.202***	0.186***
	(0.089)	(0.137)	(0.154)	(0.086)	(0.164)	(0.086)	(0.059)	(0.078)	(0.065)	(0.067)
Mining transfers per capita squared	-0.007***	-0.011***	-0.014***	-0.016***	0.004	-0.009***	-0.006**	-0.007*	-0.006***	-0.006***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.008)	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)
Log(1 + real mineral production)	-0.008*		-0.008		0.002		-0.006*		-0.004	
	(0.004)		(0.006)		(0.006)		(0.003)		(0.005)	
Never-producing districts	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean of dependent variable	0.60	0.60	0.92	0.91	1.29	1.30	0.25	0.25	0.69	0.69
Observations	16,395	14,965	16,395	14,965	16,395	14,965	16,395	14,965	16,395	14,965
Districts	1,792	1,633	1,792	1,633	1,792	1,633	1,792	1,633	1,792	1,633
Within R-squared	0.127	0.127	0.126	0.119	0.149	0.118	0.049	0.045	0.046	0.042

Notes: Outcomes are municipal employees per 1,000 inhabitants. Odd-numbered columns include the production control; even-numbered columns use the canonical never-producer classification. All models include district and year fixed effects. Standard errors are clustered by district. The RENAMU samples are outcome-specific and extend through 2010. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9. Impact of Natural Resource Booms on Municipal Expenditures by Function

	Planning	Agriculture	Social assistance	Education and culture	Energy and resources	Industry and services	Health and sanitation	Transport	Housing and urban development
Mining transfers per capita	251.450***	167.095***	30.232***	156.321***	14.949***	25.832***	90.511***	252.795***	34.643***
	(46.674)	(28.659)	(3.044)	(15.051)	(3.393)	(4.894)	(21.691)	(55.647)	(4.951)
Mining transfers per capita squared	-6.195***	-3.846***	-0.272**	-4.603***	-0.456***	-0.813***	-1.112	-7.568***	-1.024***
	(1.457)	(0.846)	(0.112)	(0.736)	(0.106)	(0.128)	(1.367)	(1.306)	(0.205)
Log(1 + real mineral production)	-1.674*	-0.066	0.142	1.902	-0.387	-0.208	0.922	-1.063	0.255
	(0.937)	(0.843)	(0.364)	(1.663)	(0.313)	(0.288)	(1.195)	(1.425)	(0.340)
Mean of dependent variable	165.53	38.97	42.93	58.25	13.86	9.28	66.99	68.74	20.40
Observations	17,031	17,031	17,031	17,031	17,031	17,031	17,031	17,031	17,031
Districts	1,792	1,792	1,792	1,792	1,792	1,792	1,792	1,792	1,792
Within R-squared	0.318	0.168	0.119	0.261	0.035	0.069	0.234	0.233	0.053

Notes: Dependent variables are executed municipal expenditures in real 2001 Lima PEN per capita. All regressions include district and year fixed effects and the production control, with standard errors clustered by district. The sample covers the available fiscal years from 2001 through 2010. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. Impact of Natural Resource Booms on Household Well-being

	Linear	Quadratic	+ production	Excluding Lima	Producing regions	Producing provinces
Panel A: Real household income per capita						
Mining transfers per capita	43.860***	115.893***	115.708***	115.732***	118.385***	127.325***
	(13.818)	(42.085)	(42.185)	(42.527)	(42.272)	(43.562)
Mining transfers per capita squared		-2.466**	-2.460**	-2.461**	-2.526**	-2.768***
		(0.999)	(1.001)	(1.010)	(1.001)	(1.037)
Log(1 + real mineral production)			3.741	3.355	3.846	3.777
			(2.957)	(3.077)	(2.970)	(2.999)
Mean of dependent variable	478.73	478.73	478.73	443.80	495.72	557.78
Observations	203,117	203,117	203,117	173,139	146,595	87,031
Districts	1,285	1,285	1,285	1,180	1,030	561
Within R-squared	0.012	0.013	0.013	0.016	0.012	0.013
Panel B: Real household consumption per capita						
Mining transfers per capita	2.303	11.719	11.615	11.127	13.404*	12.956*
	(2.320)	(7.229)	(7.262)	(7.375)	(7.102)	(7.751)
Mining transfers per capita squared		-0.322*	-0.319*	-0.308*	-0.364**	-0.348*
		(0.180)	(0.181)	(0.184)	(0.177)	(0.194)
Log(1 + real mineral production)			2.088	1.769	2.242*	2.291*
			(1.316)	(1.343)	(1.348)	(1.353)
Mean of dependent variable	401.83	401.83	401.83	377.11	411.21	445.23
Observations	203,117	203,117	203,117	173,139	146,595	87,031
Districts	1,285	1,285	1,285	1,180	1,030	561
Within R-squared	0.018	0.018	0.018	0.022	0.016	0.014

Notes: The sample consists of household heads in ENAHO from 1998 through 2010. Income and consumption are monthly real 2001 Lima PEN per capita. All regressions include district and year fixed effects; columns 3–6 include the production control. Producing regions and provinces follow the canonical permanent-producer classification. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.